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ON THE NECESSITY OF GOVERNMENT AID IN ORGANIZING A SYSTEM OF TESTS OF MATERIALS USED FOR STRUCTURAL PURPOSES.

By CHARLES MACDONALD.

A Paper read at the Washington Meeting of American Institute of Mining Engineers.

It may seem to be almost unnecessary to occupy the time of the Institute in further consideration of a question which has been so comprehensively treated in papers already on file in our own *Transactions* and in those of the American Society of Civil Engineers.

Unfortunately, however, the results of these concerted efforts have not been to materially increase our stock of knowledge in the direction sought for; and as the necessity for this information is becoming more and more apparent as the demand for structural materials increases, it is believed that by continuing the agitation by means of discussions in this and kindred societies, whose members are vitally interested in obtaining reliable data as to the properties of the materials they are called upon to work with, public opinion may be educated up to the importance of exerting such an influence upon the law makers of the country as will result in the formation of a competent board, with adequate means at its disposal, to carry out this great work in a manner alike acceptable to the makers and users of the materials in question.

It may be proper in the first place to glance briefly at what has been attempted thus far, then to indicate some of the more important lines of needed investigation, and finally to consider reasons why Government aid may with propriety be sought for in carrying on the work.

At a Convention of the Society of Civil Engineers, held at Chicago, June 5th, 1872, it was, on motion of General William Sooy Smith, resolved, that

Whereas, American engineers are now mainly dependent upon formulæ for the calculation of strength of the different forms of iron and steel, not based on experiments upon American materials and manufacture; and

Whereas, These differ greatly in many of their characteristics from those of foreign production, both in their nature and forms; therefore,

Resolved, That a committee of five be appointed to urge upon the United States Government the importance of a thorough and complete series of tests of American iron and steel, and the great value of formulæ to be deduced from such experiments.

Pursuant to this resolution a committee was appointed, by whose efforts Con-

gress was induced to pass a law, March 4th, 1875, providing for the appointment of a United States Board to Test Iron and Steel, and an appropriation of seventy-five thousand dollars ($75,000) was made for that purpose.

The board appointed under the law above referred to consisted of Colonel T. T. S. Laidley, Ordnance Department, U. S. A.; Commander L. A. Beardslee, U. S. N.; Lieutenant-Colonel Q. A. Gillmore, U. S. A.; Chief Engineer David Smith, U. S. N.; William Sooy Smith, C.E.; A. L. Holley, C.E.; R. H. Thurston, A.M., C.E., Secretary; and they were ordered to report from time to time to the President of the United States.

The first and most important duty of the board was deemed to provide an accurate testing machine. This proved to be a more serious matter than was at first supposed. There were no machines in the country which could be considered as giving anything more than approximate results; and to construct a new machine upon approved principles required much time and a large expenditure of money; much more, in fact, than was represented by the sum paid for it. At length a machine was completed, which for accuracy of the results obtained and range of power exerted, is unequaled, perhaps, in the world. Owing to the length of time expended in completing it, however, the original appropriation became exhausted, and the board was legislated out of existence, having had scarcely an opportunity to verify the capabilities of the very instrument which had been brought to perfection under its fostering care, and through the proper use of which so much valuable information could be obtained.

As might have been supposed, the board did not confine its efforts to the construction of this machine. About 150 specimens of steel were analyzed, and tests of their physical and mechanical properties made with a view to determine the relations between chemical constitution and useful qualities.

In wrought iron the effects of reheating and rerolling were carefully examined, and the report contains valuable information as to the different processes of making and rolling iron, the effects of various kinds of strain, the best methods of making cables for large vessels, and to determine how uniform strength can be secured in iron of different sizes in the bar, and how to make large masses equally strong with small pieces.

Alloys of copper-zinc and copper-tin-zinc were exhaustively examined and the results exhibited on a small triangular model from which may be obtained by inspection the characteristics of any possible combination of these metals.

Extensive preparations had also been made for ascertaining experimentally the strength of rolled beams and shape irons, for which we are now dependent almost entirely upon theoretical formulas.

Although the board had ceased to exist, the machine remained the property of the United States. It is located in the Watertown Arsenal, near Boston, under the immediate charge of the Ordnance Department of the army, and is nominally at the service of engineers and others who may be able to defray the necessarily heavy expense of working it for their own private benefit. So much for what has already been accomplished. Should the efforts now being made to revive interest in the subject prove successful, the field for investigation will be found to be most fruitful of results. To mention a few instances only: In the department of bridges there were required for last year's construction not less than 80,000 tons of Iron and steel representing, say, 50 miles of bridges, over which the safety of life and limb is supposed to be assured by the accuracy of the calculations of the designer, no less than the quality of the material employed. Of this material upwards of 35 per cent. is in the form of compound sections specially adapted to resist compressive strains; and yet until quite recently all the experimental data upon which such sections are designed were obtained through the instrumentality of testing-machines which, particularly at high pressure, are liable to give very erroneous results.

Quoting from Mr. Holley's paper on the United States Testing-machine at Watertown, alluding to C. E. Emery's device for overcoming packing friction:

"It is certainly worth many times its cost in proving the worthlessnes of hydraulic testing-machines as heretofore

constructed. The readings of the permanent weighing apparatus as compared with those of the cylinder gauge when the piston was not revolving, showed in some cases an error of 40 per cent."

It is safe to say that the recent fall of one of the most important bridges in the country would not have occurred, if, at the time of its construction, the engineer could have tested full-sized sections of his material on such a machine as the Government now owns at Watertown Arsenal.

The tension members of bridges are in the form of eyebars varying in sectional area from one inch to twenty inches. Until quite recently it was assumed that the same strain per square inch might be applied indiscriminately without regard to the size of the members, or to the amount of work done upon the material in the rolls; but the few bars which have already been tested at Watertown clearly indicate that this is a most erroneous assumption; and one of the first duties of a testing board would be to establish the law governing the diminution of strength due to increased section, and to establish the relation between ductility and ultimate strength. Then would follow tests to determine proper form of head, and such other details of manufacture as might suggest themselves.

Of rolled beams there were produced last year upwards of 50,000 tons. This form of product is used chiefly in floors of buildings, often to sustain great weight, as in warehouses, and somewhat also as stringers in bridges. Their strength is estimated by theoretical formulas in which the physical constants are taken from experiments upon foreign irons tested under circumstances entirely different from what are obtained in actual practice. Fortunately for the cuase of safety in the use of such materials it is probable that the formulas in question do not represent the full strength, and that a considerable amount of unnecessary weight is loaded upon our structures in consequence; but there is all the more reason why the actual strength should be determined by experiment, in order that an uniform factor of safety may apply to every member of a structure, or in other words, that it shall be equally strong in all its parts.

Did time permit, it would be possible to point out many other directions in which experimental knowledge is sadly needed, but if nothing else were done than to determine practically the laws which govern the strength of compression and tension members of bridges, and the flexure of rolled beams, a very great advance would be made in our modes of construction, and a greater safety would be assured to the hundreds of thousands of people who are constantly trusting their lives upon such structures.

What has been said regarding the importance of testing particular constructions applies equally to iron and to steel; but there are special reasons for investigating the properities of steel which should command attention. It is admitted to be the metal of the future, for large constructions at least; it is stronger and more homogeneous than the best iron, and owing to the substitution of mechanical appliances for wasteful muscular effort in its manufacture, there will come a time, and that before very long, when it can be furnished commercially at less cost than iron, in large quantities and of uniform quality. It only remains now to determine by a competent and disinterested authority what the general characteristics of this material are, to insure for it a continually increasing demand.

At present the finished product of the converter is principally in the form of steel rails. It so happens that the best testing-machine for a steel rail is the track, and railroad companies, by careful inspection, taken in connection with chemical analysis, are thus experimentally determining the quality of steel which answers best for that particular purpose.

For other constructions, such as bridge and ship work, very different qualities of steel are required, depending on the nature and direction of the forces to which it is subjected; and until all such questions are determined by competent and disinterested investigators, the benefits to be derived from the cheap production of steel by the pneumatic or open-hearth processes, will for a long time be confined to the favored few who are engaged in supplying the demand for steel rails.

It is hoped that enough has been said

to establish the fact that a producing class of the community stands in want to-day of certain scientific information, which, if obtained promptly and in a manner to command universal acceptance, would tend to improve and enlarge one of the staple industries of the country. From the nature of the case such information can best be obtained by the assistance of the general government. Shall the effort be made to secure such assistance?

It may be asked, why should the United States Government appropriate money for the purpose of making experimental investigations which might as well be undertaken by those who are immediately interested? In reply to to this, the following quotation from the memorial recently presented to Congress by the American Society of Civil Engineers will commend itself:

"And your memorialists further represent that there is no prospect that the necessary tests will be made without the aid of government. Should private manufacturers or builders test their own materials they might not give the public the benefit of their experiments; such experiments would not have that assurance or impartiality and that high authority which those made under the authority of the government would have. Experiments conducted by private parties would be so different in the objects, methods, and circumstances of applying tests as to render it impossible to properly collate and verify them; they would therefore be of comparatively little value in ascertaining accurate general results."

I am aware that it is often a difficult matter for legislators to draw the line between public and private interests, and that in the multiplicity of claims made upon them they must be expected to look doubtingly upon anything that calls for money; but it would seem that where such enormous revenues are derived by the country from the effort to secure the exclusive consumption of American manufactures of iron and steel, it would be asking no more than justice for the users of these materials that the government should lend substantial aid in determining their general characteristics.

Again, the government of the United States is in possession of a most important element in the problem, the testing-machine already referred to; it represents a very considerable expenditure in money and years of patient labor, which, it is safe to say, would never have been expended had there not been a well-grounded hope that an amount of knowledge would be obtained through its instrumentality which would contribute largely to the general good.

In its present shape this machine is utterly unable to meet the wants of even such private demands as are made upon it. I am informed by an engineer now engaged in the construction of one of the most important bridges in the country, that he recently sent to Watertown nine steel eyebars to be tested, and it required seven and a half days to make the tests, while the cost to his company was at the rate of $15 for each bar. This is admitted to be due to the fact that there are no means at the disposal of the department wherewith to engage an efficient permanent staff of assistants to handle the specimens promptly, and the result is that a most valuable instrument for scientific research is allowed to remain in comparative idleness for the want of a few thousand dollars.

As to the most effectual means of expending government aid in the direction sought, there may be difference of opinion, but all are agreed as to the necessity of obtaining results which may be accepted as authority alike by manufacturers, builders, and engineers. This could be accomplished either by the appointment of a special committee, similar to the one created under the law of March 4th, 1875, with an adequate appropriation to purchase materials and make a comprehensive series of tests; or failing in this, a moderate sum of money might be placed at the disposal of such an institution as the one under whose auspices we are now assembled, to be expended in testing such constructions as would be furnished from time to time by engineers and others in their regular practice, with the understanding that all information thus obtained should become public property by regular publication in the *Transactions* of this and kindred societies. Could we feel assured of the permanence of a special commission, the members of which could devote the nec-

essary time to the work, this would doubtless be the most satisfactory to a large majority of those interested.

There are uncertainties, however, connected with all such special legislation in a government constituted as ours is, that should be carefully considered in this connection lest we should be compelled to undergo a similar experience to that which befell the previous board, which, from no fault of its own, was brought to an untimely end after having perfected the means by which, for the first time, really accurate testing could be done in this country.

It is to be hoped that eventually a Department of Public Works will be instituted, having a co-ordinate power with other departments, as of the Interior, for example, to which all questions relating to the expenditure of public money, either for internal improvements or for scientific investigations connected therewith, may be referred, and through which the interests of the producing classes, including engineers, builders, and manufacturers, may receive that special consideration which their importance demands.

Whatever method may be adopted will be liable to defects as a matter of course. We must be content to go slowly and surely, to be patient and judicious in advocating our claims, and above all to bear in mind that if our cause is a good one, as we believe it to be, and we do not succeed in impressing its importance upon Congress, it will, in all probability, be our own fault.

REMARKS OF GENERAL MEIGS.

I do not know that I can do any more than to express my entire concurrence in the views which have been already expressed by Mr. Macdonald. It appears to me that he has gone over the whole subject. I might add in regard to appealing to the government for an appropriation, that the government itself is the largest single user of these materials; the railroads together use more, but there is no single organization which uses so much. Congress appropriates the money with which are builded the large government structures that are found now in almost every city. It is stated in the public press that it is contemplating the erection of a hundred new government buildings in a hundred cities this year. In all these buildings the floors are supported upon rolled iron beams, and the principal materials used for roofs are iron. These buildings are all dependent for their cost upon the size of their dominant members, and, as a consequence, upon the factor of safety which the engineer allows; so that as long as there is uncertainty as to the proper coefficient of safety, perhaps from two to five times as much metal as is actually necessary may be put into these members. There are other materials used in buildings,—brick, stone, marble, timber,—but these materials we buy by the cubic yard or cubic foot, they are comparatively inexpensive; metal we buy by the pound and at this time we pay pretty high prices for the pound; so that if we can reduce our general coefficient of safety, we save perhaps one-half to two-thirds of the actual cost of the material used. Congress sits under a roof of iron, its building is crowned by an iron dome; it is about building a new navy and is considering whether it shall be of steel or of iron, and the result will depend upon the comparative qualities of steel and iron. I see it stated by a gentleman, eminent in the actual practice of steel making, that his company is prepared now to furnish steel which shall be guaranteed a tensile strength of 60,000 pounds to the square inch, with 30 per cent. elongation. One can hardly imagine a more admirable metal.

Therefore I think that this society can with a good heart go to Congress, and if they can only convince some of its leading members of the necessity of more knowledge on this subject, it appears to me they must meet with success.

REMARKS OF MR. T. C. CLARKE.

The history of iron construction in this country well illustrates the three phases of thought described by Auguste Compte, the French philosopher.

The first is the era of faith, when belief in the safety of structures rests on the authority of the designer. The second is the era of criticism, when plans of structures are analyzed with much mathematical skill, but the data upon which the chain of reasoning depends is assumed upon insufficient experiment. The third, upon which we are now enter-

ing, is a scientific era which demands experimental proof. It also demands that this proof shall be derived from experiments made on full-sized specimens, such as are in actual use, and not upon toy models.

Until the construction of the United States testing-machine, now at Watertown Arsenal, it was impossible to make such experiments with accuracy. We now have a machine in which we can test full sized specimens of every part of a bridge or other structure that we want to use, and under the same conditions in which it is actually used. The next thing is to get money to make these experiments available. No private individuals can afford to do it, and even if they could, they might wish to keep the results to themselves. So that the next point is that we want money, and that I believe everybydy thinks we should ask Congress for it. We want also, as has been said, some one who shall make a business of testing, and who has plenty of time. Persons who are employed in private business are too much in a hurry, they want to do a thing and get done with it, and then do something else; but government officers are entirely free from this feeling; time to them is of no account, and in experimenting that is the very element that is of value; it does not do to be hurried; the great thing is to get it right and to test your results, and go over it again and again. And the experimenter who operates the machine must be some person educated up to the use of it. We then want a general advisory board who will indicate a plan of experiments, collect the results, and publish them. Some experiments were made the other day at the Watertown Arsenal upon full-sized Phœnix columns. Any one can see at once that these are very valuable experiments, because we have certain columns all of the same quality of metal, the same workmanship, and the same cross-sections, and differing only in length. As far as these columns are concerned this would be all, but it would then suggest itself that we make experiments with the same columns alike in other respect but with different cross-sections, and then test them made of steel, and so on. The engineer is often asked why don't you use steel? We can't expect to know anything about it at all until experiments are made in the way that I have indicated in some such machine as this. I venture to say that Messrs. Fowler & Baker, who expect to build the great bridge over the Firth of Forth, in Scotland, cannot find out anything about the strength of the parts of their structure, unless they have a machine equal to our government machine. Then, the last thing of all, after having made the experiments, they ought to be published monthly and sold in all book-stores. Then every engineer could get a report, and would have questions to ask and suggestions to make, and would at once write to the board and give them the benefit of his thoughts. These suggestions would be one of the most valuable results of prompt publication.

REMARKS OF MR. O. CHANUTE.

In discussion of Mr. Macdonald's paper, I can say little more than to add to the general acknowledgments of ignorance, and like several of the gentlemen who have preceded me, make one of those confessions which are thought to be good for the soul.

Having had some experience in the erection of bridges during past years, I am aware that we yet need much information in order to proportion them to the best advantage.

I would more especially like to emphasize three of the points mentioned by Mr. Macdonald, as among those upon which we lack knowledge; these are: first, the behavior of steel: second, the proportions of compression members; and, third, the influence of the size of a bar upon its strength per square inch.

First, as to steel. While we all acknowledge this as the material of the future, our position may be said to be still one of expectancy. Few engineers are bold enough to employ it largely in bridges, and those who do, find such serious difficulties in obtaining uniform grades of it, are so puzzled by apparent anomalies and unexpected phenomena, that it requires considerable faith and courage to apply it in large structural masses. A series of systematic experiments, such as have been partially made by various European nations in their government shipyards and elsewhere, by which we should be enabled to con-

nect the influence of the chemistry of steel and of the process of its manufacture, with results of the various modes of working the product into its final shape, would doubtless add so largely to our knowledge of modern structural steel, as to make reasonably clear much that we now only suspect, and give us the necessary knowledge and confidence to avail ourselves of the increased strength and economy which this metal promises. At present we know that the strength exists, but we also know that steel is brittle under many conditions; and where human lives are at stake, where failure would involve such disastrous consequences, we dare not avail ourselves of the strength of that metal, unless reasonably sure that it will not break.

Second, as to compression members of structures. They are now proportioned upon formulas which were framed many years ago in England, and which were based upon very few experiments, some thirty in number, if I recollect rightly. Not only were those experiments tried upon pieces materially smaller, and of different shape from those which we now generally use, but they were made with English irons, which are found to differ in some respects from the characteristics of American irons. We have accordingly made some changes in the constant numerical factors of the formulas, to attempt to adapt them to our use, but we now find from the experiments recently made at Watertown with the government machine, for Messrs. Clarke, Reeves & Co., that even the modified formulas are erroneous, and do not agree with the actual condition of affairs. In fact there is great uncertainty as to the actual strength of the bridges which we are now daily erecting. Their strength is of course limited by that of the weakest part, but while we endeavor to make every part equally strong, as well as we know how, yet we are almost entirely ignorant as to what is actually the weakest part of a bridge of any magnitude, and of just where it would give way first, if loaded to rupture.

While no man knows exactly what weight will crush flat, say a 4-inch cube of wrought iron, we do know that it begins to yield, without recovering its shape, at pressures of some 36,000 to 40,000 pounds to the square inch. Accordingly, with the aid of the formulas I have mentioned, we proportion compression members for an assumed crippling point, varying from, say 35,000 pounds to the square inch, for pieces of ten diameters in length, down to about 24,000 pounds to the square inch for pieces forty diameters in length, and upon these we allow strains varying from 7,000 to 4,800 pounds to the square inch, as working compressive loads; while in tension we allow some 10,000 pounds to the inch on iron, with a breaking strength of 46,000 to 50,000 pounds, and an elastic limit of 26,000 pounds per square inch.

Now, in my judgment, the crippling point of a compression piece corresponds more nearly with the elastic limit in tension, than with the ultimate or breaking strength. The probabilities of any compression bridge member being strained up to the crippling point, are nearly as remote as the probabilities of a tension member being strained up to its elastic limit, and to have all parts equally strong, should experiments justify this view, we should base our assumed margin of strength (you will note that I do not use the term "factor of safety," as I think it misleading), upon the *crippling* strength and the *elastic limit* of the material. As for myself, I believe that we are now making our compression members considerably stronger than the tension members; that if we were to break down a bridge by fair loading, granting of course that all the *connections* should be made stronger than the body of the pieces they attach together, rupture would probably first take place in one of the tension members. But then while so believing, I do not *know*. I confess my ignorance upon this point, and until this ignorance is removed, I shall go on specifying for proportioning bridges in the old way, and with the old formulas.

Third. Not only is there great uncertainty concerning the actual strength of compression members, but we do not know accurately the strength in tension of full-sized bars worked to various dimensions and with a different amount of pulling and squeezing in the rolls.

In the bridge specification of the New York, Lake Erie and Western Railroad, we require that full-sized pieces of flat,

round or square iron, not over 4½ inches in sectional area, shall have an ultimate strength of 50,000 pounds per square inch, and stretch 12½ per cent. in their whole length, while for bars of a larger sectional area than 4½ inches, we allow a reduction of 1,000 pounds per square inch, for each additional square inch of section, down to a minimum of 46,000 pounds per square inch. This was adopted after consultation with various manufacturers of iron, who had large experience; but the discrepancies between the data which they furnished, and the views which they expressed when the proofs of the specifications were submitted to them, showed clearly that they did not agree as to results, and that they too were in need of further experiments upon full-sized members of various dimensions.

In the government machine at Watertown, we have for the first time in this country, a machine adequate to obtain correct results upon full-sized members. It has a capacity of 400 tons, while former machines at various bridge works had a capacity of only 150 tons, and could not be trusted to work accurately, to even 100 tons. Tension members being composed of several parallel bars, could be tested in detail, provided the dimensions of the bars did not exceed say 8 inches by 1 inch, but compression members, with a sectional area of say 12 to 20 square inches, could not be tested at all, and resort had to be had to small models, which, as already stated, are not found to give the same results as full-sized pieces.

Tests are made for two purposes; first, to ascertain the best *form* in which the metal can be placed to resist the strains; and, second, to ascertain the *quality* of the metal itself. Upon the latter point experiments are being made every day by manufacturers, bridge builders, and corporations which are erecting structures. Every time we contract for a bridge we test many specimens of the materials which go into it, and the corporation with which I am connected has tried hundreds of experiments upon the quality of the metals it has used, which will be very much at the service of a testing board, should one be appointed. These experiments have been carried as far as we had any interest, that is to say, to the point of ascertaining the quality of the metal furnished; but we have preserved many of the specimens, and a testing board could ascertain the chemical constitution of each, and, perhaps, be enabled to connect the various behavior of the specimens with their chemical characteristic and the process of their manufacture.

For information as to the best *forms*, however, we must rely upon the government machine, and especially upon government aid, as no single firm or corporation has sufficient interest at stake to warrant it in planning and paying for the great cost of a systematic series of experiments, to ascertain what are absolutely the best shapes into which to put the members (chiefly those of compression), by testing full-sized pieces. Moreover, if any firm or corporation were to become possessed of information which is so much needed, it would probably endeavor to give it commercial value, and to recoup its expenses, to say the least, by keeping such information for itself as long as it could, and the general public of metal users would remain in its present ignorance.

It seems to me, therefore, that the general government is the proper party to institute and carry out the needed experiments, not so much because, as has been claimed, the materials to be tested are "American" iron, steel, and other metals, but because there is need of general information, which no single other party is likely to obtain and make public. The government has the machine, it has abundant resources, and the manufacturers and engineers of the country, with universal good will, stand ready to tender their aid and technical knowledge.

Now one word as to the organization of the inquiry and the doing of the work. There should be some general plan of operations, and this would probably be best evolved by the deliberations of a commission, but the actual work will be chiefly done, as I think, by one man, that is to say, by the man who may be placed in general charge of the experiments, and whose duty it will be (to draw an analogy from industrial organizations) to act as chief executive officer, or superintendent if you will, and to plan and draw deductions from the various

needed experiments. The commission, if commission there be, may lay out the general plan, but it must have some one head in charge of the actual carrying of it out.

But how shall we secure the selection of the very best man to put into that position? He may be appointed in many ways. He may be selected by the President of the United States, or by the Secretary of War, or by the Secretary of the Navy, or by the head of one of the government bureaus, or by the commission which has been suggested, and which would thus act (to refer again to industrial organizations) as a board of directors or trustees. It does not, in my judgment, make much difference how he is selected, provided we get the right man. A mistake may be made at first, and changes may have to be made, until the right man, a man like Kirkaldy, in England, is brought forward, who shall possess the necessary technical skill, the executive ability, and the high standard of accuracy and thoroughness to conduct the experiments, as well as the talent to deduce general conclusions from them.

Upon the whole, I believe that the best way of selecting such a man, would be through a board of commissioners. This plan has been found to work best for joint-stock companies carrying on large operations, and I hope that Congress will organize the work through a commission as prayed for in the memorial of the Society of Civil Engineers.

THE UNIVERSAL THEOREM,*

FOR THE INVOLUTION AND EVOLUTION OF POLYNOMIALS.

By GEORGE H. JOHNSON, B.S.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THAT mathematicians have recognized the need of a general theorem for raising any polynomial to any power, is evident from the various attempts which have been made to find an easy method of writing the powers of polynomials, without using the tedious process of multiplication. The tables of numerical coefficients which have been obtained empirically; the general term in the expansion of the n^{th} power of any polynomial, as given by Todhunter, Hackley, and others; and the adaptation of Arbogast's theorem to algebraic involution as given by Galbraith and Strong, show what has been done in this direction. That these attempts have not been sucessful in attaining simplicity and utility is evident from the fact that no reference is made to them in many standard treatises on Algebra.

After careful study I have deduced the laws of formation of the n^{th} power of any polynomial, and have expressed them in a theorem which is both simple and explicit.

I believe that a brief examination is sufficient to show the decided superiority of this method.

Great simplicity is attained by arranging the answer in the form of an entire function, as the coefficients are repeated as many times as there are terms in the given polynomial. It will be seen by examining different examples that the use of the theorem saves about 75 per cent. of the work of multiplication, and about 50 per cent. of that required when substitutions are made in the binomial formula. When the polynomial contains a large number of terms, or the power is high, the advantage in using the Universal Theorem is even greater. Suppose that we desire the fourth power of a polynomial containing ten terms.

The required expansion contains seven hundred and fifteen terms, which may be

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* The following extract is taken from the report of the committee who examined the theorems:

NEW BRUNSWICK, N. J., June 19, 1882.

The Knickerbocker Prize for Original Research has been awarded to George H. Johnson of New Brunswick for his paper on "The Universal Theorem." The subject is one which has exercised the powers of the ablest mathematicians, and the accomplished expert who examined it says that "it is *clear* and *complete*, and no doubt is entirely original. The theorem is given a convenient form for practical work, both as a *formula* and a *rule*. It is a general theorem of which Newton's Binomial Theorem is a particular case. I regard it as a *very* highly meritorious production."

GEO. H. COOK, WILLIAM J. R. TAYOR,
DAVID D. DEMOREST, *Committee.*

written down immediately by using the theorem. If worked by multiplication, taking the square of the square, we must use three thousand nine hundred and seventy terms. If we use the Binomial Theorem we must make eight substitutions and use over two thousand terms. In this case we see that the Binomial Theorem saves less than one-half of the work of multiplication, whereas the Universal Theorem saves nearly five-sixths of the work.

I have used the same method to discover the theorem which Sir Isaac Newton used to obtain the Binomial Theorem. That is, I have compared a great many developed powers in order to discover the laws of formation. I have denominated the theorem "Universal" because it may be applied to the involution and evolution of any algebraic expression. From the Universal Theorem may be deduced an infinite number of special theorems. Indeed, we may deduce from it several series of theorems, each series containing an infinite number of special theorems. As the first case, we have the expansion of any binomial to the n^{th} power, which is Newton's Binomial Theorem. We may obtain in the same way trinomial and quadrinomial theorems.

We may have a second series for raising any polynomial to any specified positive integral power; for example, to cube any polynomial.

We may have a third series the same as the preceding, except that the exponents of the required powers are negative. Finally, we have two more series in which the exponents of the required powers are positive and negative fractions. By making the exponent of the required power, minus one, I have obtained a theorem for writing the reciprocal of any polynomial. I have also made several numerical applications of the Universal Theorem, and have thus found an abridged method for obtaining the squares and cubes of numbers.

STANDARD MEASUREMENTS.

By GEORGE M. BOND, Hartford, Conn.

Transactions of the American Society of Mechanical Engineers.

The subject of standard measurements is not a new one, though it has received the attention of minds well qualified to master it; still, the lack of a definite system of uniform sizes for general use, especially in machine construction, led to the appointing of a committee by the Master Car Builders' Association to select some one prominent firm engaged in tool-making, to undertake to furnish standard United States, or "Franklin Institute" thread screw gauges.

The choice fell to the Pratt & Whitney Company, of Hartford, Conn.; and in order to commence aright, the services of Professor W. A. Rogers, of Harvard College Observatory, Cambridge, were enlisted for the purpose of obtaining an exact transfer from the British Imperial Yard, thus enabling the company to feel assured that the "bottom" had been reached, and to do, once for all, and for the benefit of all, what seemed absolutely necessary for a correct beginning.

The necessities growing out of the difficulties of subdividing the yard, and of applying such subdivisions in practice, led to the construction by them of a comparator, of the form which Professor Rogers found best adapted to comparison of standards. Two of these comparators, or "measuring machines," have been made; one to be placed in position at Harvard College, and the other to remain at the works of the company for use in future comparisons.

It is not the intention in the present paper to give an exhaustive report, or a detailed account of the condition, at this late day, of the question of standards of length, but simply to furnish, in a brief and general way, such facts and statements regarding the subject as are of importance to those interested in the adoption of a uniform standard of size in the manufacture of tools and machinery requiring interchangeability of parts, and to show in what the standard for the

basis of future measurements consists, and the method adopted for determining how closely in practice such standard measurements may be applied.

As is well known, three natural units have been proposed as the basis of standards of length, as follows:

I. The length of a pendulum beating seconds in a vacuum, at the level of the sea, in the latitude of London.

II. One ten-millionth part of the quadrant of the earth's circumference.

III. The length of a wave-length of given refrangibility.

The first of these natural units was found to be unsuitable for the accurate restoration of the original British Yard, rendered useless by the great fire, October 16th, 1834, which destroyed both houses of Parliament, where the standard had been kept.

Sir Francis Baily, Bessel, Kater, and Dr. Young found serious errors affecting the comparisons originally made between the bar marked "Standard, 1760," and the exact length of a pendulum beating seconds under the above conditions.

It may be interesting to here insert the act legalizing the standard:

"SECTION 1. Be it enacted . . . . that from and after the first day of May, one thousand eight hundred and twenty-five, the straight line or distance between the centers of the two points in the gold studs in the straight brass rod, now in the custody of the clerk of the House of Commons, whereon the words and figures "Standard Yard, 1760," are engraved, shall be, and the same is hereby declared to be, the original and genuine standard of that measure of length or lineal extension called a Yard; and that the same straight line or distance between the centers of the said two points in the said gold studs, in the said brass rod, the brass being at the temperature of sixty-two degrees Fahrenheit's thermometer, shall be, and is hereby denominated the Imperial Standard Yard.

* * * * * *

"SEC 3 And whereas it is expedient that the said Standard Yard, if lost, destroyed, defaced, or otherwise injured, should be restored to the same length by reference to some invariable natural standard; and whereas it has been ascertained by the commissioners appointed by His Majesty to inquire into the subject of weights and measures, that the said Yard hereby declared to be the Imperial Standard Yard when compared with a pendulum vibrating seconds of mean time, in the latitude of London, in a vacuum at the level of the sea, is in the proportion of thirty-six inches to thirty-nine inches, and one thousand three hundred and ninety-three ten-thousandths parts of an inch.

"Be it therefore enacted and declared, that if at any time hereafter, the said Imperial Standard Yard shall be lost, or in any manner destroyed, defaced, or otherwise injured, it shall and may be restored by making a new Standard Yard, bearing the same proportion to such pendulum as aforesaid as the said Imperial Standard Yard bears to such pendulum."

In view, therefore, of the errors due to the doubtful reductions of the level of the sea, and the estimated specific gravity of the pendulum employed, and also to other important factors, shown conclusively by Dr. Young, Kater, Bessel, and Baily, to be unreliable, the method adopted and employed in restoring the Imperial Yard, was to use standards which had previously been compared with it.

The bars available for this purpose were:

(*a.*) Shuckburgh's scale (0 — 36 inches).

(*b.*) Shuckburgh's scale, with Kater's authority.

(*c.*) The yard of the Royal Society, constructed by Kater.

(*d.*) The Royal Astronomical Society's brass tubular scale.

(*e.*) Two iron bars, marked A_1 and A_2, belonging to the Ordnance Department, and preserved in the office of the Trigonometrical Survey.

The restoration of the standard was intrusted to Sir Francis Baily, but his death occurring soon after, the work of restoration was committed to the Rev. R. Sheepshanks. Baily had, however, made numerous experiments regarding the proper material to be used, and that now adopted is known as Baily's metal, the composition of which is: copper, 16; tin, 2.5; zinc, 1.

The mean of all the observations taken, in comparing these available standards, led Sheepshanks to assume that "Brass Bar 2," the name given to the working or provisional standard employed in his investigations, was equal to 36.00025 inches, in terms of the lost Imperial Yard, at 62° Fahrenheit.

The Imperial Standard Yard, known as "Bronze 19," or as now denominated "No. 1," was then constructed according to this equation. It was made of Baily's metal, and of the following dimensions:

Length, 38 inches; width, 1 inch; depth, 1 inch.

Gold plugs are inserted in wells sunk

one-half the depth of the bar. The graduations are upon these gold plugs.

"Bronze No. 1" is the national standard yard, and is kept in what is known as the "Strong Room" of the Old Palace Yard, in London.

Besides this bar, four Parliamentary copies were made, one copy being kept in the Royal Mint, one in charge of the Royal Society, one at the New Westminster Palace, and the other at the Royal Observatory at Greenwich. Of the forty copies prepared of Baily's metal for distribution to foreign governments, only two are exactly standard at 62° F.,—"Bronze 19" and "Bronze 28,"—"Bronze 28" is kept at the Royal Observatory, as an accessible representation of the national standard.

All the other copies have the temperature, at which they are standard, marked upon them.

In 1856 "Bronze Bar No. 11" was presented by the British Board of Trade to the United States; at that time it was declared to be standard at 61.79° F. According to recent comparisons this bar is *now* .000088 inches shorter than the Imperial Yard No. 1.

In reproducing a standard bar, whether for reference, or as a *working* standard, line or end, measure, or both, care must necessarily be taken to know *positively* that the surface, upon which the lines are ruled, is a plane surface, in other words, to avoid the slightest amount of flexure, which would obviously vary the distance between the lines, especially when these lines are upon the outer surface of the bar, and hence, in supporting a bar, the points of support have been found by Sir George Airy to be the distance apart represented by the formula:

$$\frac{\text{Length.}}{\sqrt{n^2-1}}$$

"n" being the number of supports. When there are two supports this formula gives 10.39 inches for the distance between the supports in the case of the yard bars, and 28.87 centimeters in the case of the *meter* bars.

Placing the gold plugs at the bottom of the wells, sunk half-way into the bronze bar, was intended to overcome the difficulty of flexure, as the lines would then be at the best plane of variation caused by flexure, still, by placing the bar upon supports in such a way as to neutralize this tendency of bending, and having the surface carefully worked to a plane under a microscope of a high power before the lines are ruled. This difficulty is removed if the lines which are subsequently traced remain in focus throughout the entire length of the bar.

Professor Rogers' method of using a mirror surface of mercury as a reference plane for working the guiding surfaces or "ways," on which the microscope plate slides, is that adopted, and the use of a microscope of high power gives a very accurate result, the perfect focus obtained along the entire length of the mercury trough, proving conclusively that the microscope plate moves in a true plane.

In the new comparator constructed by the Pratt & Whitney Company, under the direction and from plans suggested by Professor Rogers, the means for overcoming objections and difficulties arising from errors due both to horizontal and vertical curvature, deflection, etc., are fully provided for.

The plan adopted for securing accurate sliding motion of the microscope plate is perfect *line-bearing*, and the uniform pressure is due to gravity simply, and the bearing surfaces, or guides, are such that errors due to imperfect straight-line action may easily be remedied.

The flexure of the guides is also provided for by supports placed at about one quarter the distance from each end of the guide-bars, which are heavy hardened-steel tubes, ground perfectly true and parallel, using counter-weights to overcome the flexure arising from their own weight and the weight of the moving microscope plate.

The bars used as standards by the Pratt & Whitney Company comprise:

I. A bronze bar of Baily's metal, having lines ruled on sunken gold plugs. It is a yard measure, with subdivisions into feet only. This bar is designated in the official report as "P. & W."

II. A bar of Baily's metal, identical in composition, and having the same section as "P. & W." It is 42 inches long, and has lines ruled on the surfaces of plugs carefully inserted, made of an alloy of platinum and iridium; these plugs are $\frac{3}{32}$ of an inch in diameter, and are polished to a mirror surface. This bar has

lines representing the yard at 62° Fahrenheit, with subdivisions to feet and inches, and the meter at 62° Fahrenheit.

The alloy of platinum and iridium gives clear smooth lines when ruled with the finest diamond edge, and in order to prevent accidental defacing, or injury from any cause, the lines are covered with disks of glass $\frac{1}{100}$ of an inch thick. This bar is denominated, in the report, "P. & W_2."

III. A yard and meter bar, of hardened steel, on the upper polished surface of which are ruled lines corresponding to those upon "P. & W_2," but having, in addition, *end* measure for the yard at 62° F., and for the meter at 32° F.

The neutral points of support, *i. e.*, those of least flexure, are left as "spots" on the under side of this bar, so as to avoid mistakes due this cause when in use. This bar is marked "P. & W_3."

IV. A steel yard and meter bar, untempered, but having the same form as the preceding, the only difference being that the yard and its subdivisions, and also those of the meter, are ruled upon the mirror surfaces of hardened steel plugs, the *end* measure for the yard and meter also being determined by plugs of the same material, fitted in each end, and protected from injury by an extension of the upper surface. This bar is designated "P. & W_4."

After the preparation of these bars at the works of the Pratt & Whitney Company, they were forwarded to Professor Rogers, at Cambridge, for the purpose of receiving the graduations. An additional bronze bar, the exact duplicate of "P. & W_2," was also sent, on which a provisional transfer of the yard from the steel bar in his possession was made, after applying the reduction to the Imperial Yard given by Mr. Chaney, the Warden of the Imperial Standards. This provisional bar was then forwarded to Washington, Professor Hilgard having kindly consented to compare it with "Bronze 11."

According to the report of Professor Hilgard, this yard is .000025 inches shorter than "Bronze 11."

The yards traced upon "P. & W_1" and "P. & W_2" were obtained from this provisional yard. They were then sent to Washington for final comparison with "Bronze 11."

According to the official report of Professor Hilgard, after allowing for the known relation between "Bronze 11" and the Imperial Yard, "P. & W_1" is .000053 inches longer than the Imperial Yard, and "P. & W_2" is .000036 inches shorter than this unit.

The yards and meters upon the steel bars were derived from "P. & W_1" and "P. & W_2" after the reduction of the relative co-efficient of expansion between bronze and steel.

V. A hardened-steel six-inch bar, one-half inch square in section, having upon its upper polished surface, lines ruled four separate inches, also lines representing—counting from the end of the second inch—the lengths corresponding to the *bottom* diameters or "tap-sizes" of the United States or Franklin Institute standard screw-threads, from a quarter inch to four inches.

Besides this band of irregular spaces are ruled two inches in sixteenths and two inches in twentieths of an inch; also, a band of two inches at twenty-five hundred per inch, the latter being used in the investigation of the irregular lengths or "tap-sizes."

This six-inch bar was ruled at the American Watch Factory, Waltham, upon a dividing engine constructed by the Watch Company, from designs furnished by Professor Rogers, for his use in producing standards of length. The accuracy of the settings, and the remarkable freedom from error found, upon a rigid investigation subsequently made, prove the excellence of the workmanship in the construction of the machine.

It having been found necessary to regraduate this bar to accommodate the sizes for larger diameter thread-gauges than was at first intended, a complete new series of irregular lengths was made, the new lines being ruled as nearly .001 inches apart as it was possible to set the diamond.

Upon comparing results the variation was found to be less than .00005 inches from the constant interval between the new and the old lines.

When it is considered that nearly four weeks had elapsed since the original ruling was done, and that the same settings were used, the extreme accuracy of the screw of this machine may be appreciated.

The lines upon this bar are less than .000066 inches in width, the cross-line in the eye-piece of the microscope being usually brought to cover either the *edge*, or the *middle* of the furrow made by the diamond cutter.

End-measures of hardened steel of the same brand as the hardened screw gauges have been made from a quarter of an inch to four inches, vary by sixteenths, and corresponding to the lines upon the six-inch bar. With this bar, the problem of maintaining uniform sizes in actual use is a very simple one.

The practical difficulties met with in using microscopes of high power, where extreme accuracy is necessary, render the use of any form of reflector very objectionable, as the reflected image is often distorted.

In the use of Tolles's illuminator, in which a prism is inserted within the objective of the microscope, this difficulty is obviated, giving sharply-defined lines upon opaque surfaces, such as steel or bronze, and especially upon the plugs of platinum and iridium.

The two objectives used upon the comparator belonging to the Pratt & Whitney Company were furnished to order by Mr. R. B. Tolles, of Boston, and both have this form of illuminator attached.

Referring back to the second natural unit for establishing a standard of length —that of using the ten-millionth part of the earth's circumference—the result of the labors of a commission appointed by the French Government was four iron bars, the ends carefully ground until exactly comparable with each other, and each having the required length. One of these original bars, bearing the stamp of the commission, is now in the possession of the United States Coast Survey. From these bars the present meter of the archives was constructed.

Of the third and last unit proposed—that of a wave-length of given refrangibility—it is doubtful whether this as a unit can ever be successfully adopted for general use; since the measurements of wave-lengths for an entire meter vary so much as to make the total length of a yard or meter known to a far less degree of accuracy than can be assigned to the comparison of different standards.

In conclusion, then, whenever the yard with its subdivisions is adopted as the measure of length, the unit to which all measures must be referred, is the bronze bar deposited in the "Strong Room" of Old Palace Yard, London, and known as the "Imperial Yard, No. 1."

I quote Professor Rogers's statement regarding the existing metric standards:

> "Wherever the metric system has been adopted, either by legal enactment or by actual use in the absence of definite legislation, the platinum end-measure meter deposited in the archives of Paris, is the only ultimate standard of reference."

The method adopted for the accurate subdivision of the yard and meter upon the comparator of Professor Rogers's design, is to compare the arbitrary or trial divisions first, by finding their relation to each other, with a fixed distance between immovable stops, and noting the time-worn axiom, that "things equal to the same thing are equal to each other." The yard or meter being correct in total length, the differences from the mean form an algebraic sum, the value of which is evidently equal to zero.

The micrometers for use in the standard work by the Pratt & Whitney Company were furnished by James Queen & Co., Philadelphia, and bear the name of "J. Zentmayer" as a guarantee of their excellence.

The coefficients of expansion of both the bronze and steel bars, tempered and untempered, in the possession of the company, have been carefully determined by Professor Rogers, the investigation covering a period of nearly two hundred days, under every possible condition of temperature, in air, and immersed in water, and the changes due to differences of shape or mass have been carefully noted. The changes of temperature of the bar must affect the mass throughout uniformly, and ordinarily from six to twelve hours is necessary to allow these changes to be effected before the comparison is made, the temperature meanwhile having been kept as nearly constant as possible.

I may add, in conclusion, that the standards in the possession of, and used by Professor Rogers, comprise:

(*a.*) A nickel-plated hardened steel bar, the lines upon the nickel surface having been compared directly with the Imperial Yard by Mr. Chaney, Warden

of the Standards at London, during the visit of Professor Rogers in England.

(*b.*) An end-measure Coast Survey yard kindly loaned by the Stevens Institute of Technology, of Hoboken, N. J.

The Coast Survey yard has been compared directly with the "working" yard of the Exchequer by Mr. Chaney.

(*c.*) A meter, line-measure, the lines traced upon the middle surface of an X-shaped copper bar, of small mass, this form having been adopted by the International Bureau of Weights and Measures.

This bar was traced for Professor Rogers during his visit at Paris, in February, 1880, by M. Tresca, and is signed by him.

(*d.*) A steel end-measure meter, made by M. Froment, of Paris, and declared to be 8.43 mikrons (about .00033 inches) longer than the meter of the archives.

As was mentioned at the beginning of this paper the intention is simply to report progress, and to show how far the "vital" part of this subject of standard measurements has been carried.

That part of the work which may be regarded as completed is the determination of the entire length of the yard as represented by the bars "P. & W_1" and "P. & W_2," since according to the report of Professor Hilgard, the mean of the two yards differs from the Imperial Yard by a quantity less than the *certainty* with which such comparisons can be made, viz., .00001 inches.

All the work so far described has been done with a comparator having some faults in construction, and although the errors due to imperfections have been allowed for, still it has been deemed wise to defer the publication of the full report of Professor Rogers until all the other measures have been verified by observations with the new comparator. It is confidently expected, however, that no errors of appreciable magnitude will be found in the working six-inch bar, upon which all the standard gauges depend.

A NEW DIRECT PROCESS.

From "Iron."

The following is the translation of a report, by Professor Särnström, on experiments made on dephosphorization in a charcoal furnace at Nyhamm, on the Vesterbergslagen, one of the largest iron deposits in Sweden. As is well known, bar iron was in earlier times produced from the ores by smelting with charcoal in small stoves or furnaces, and although the ores then used contained a considerable amount of phosphorus, this circumstance did not affect the mechanical properties of the metal, as most of the phosphorus was absorbed by this process in the slag. This process has been termed, by the Swedes, Osmund, and, by the Spaniards, Catalan smelting. Although excellent iron was produced by this method, it has, of course, given way to the blast and puddling furnaces. The reason of this is that in the old Swedish furnaces (in certain respects an improvement on the Spanish) the process was intermittent; it was necessary to heat and reheat them for any small quantity of iron charged, and to blow out and refill the shaft each time. It is evident that in this way a great deal of fuel was wasted, while but a very small quantity of iron was produced; and we may suppose that the desire to improve the method gradually led to the now existing mode of making pig-iron, which, as a continuous process, naturally produces a larger quantity of metal, whilst a considerably smaller quantity of fuel is consumed. In the blast-furnace it became, however, necessary to make use of ores containing only a small quantity of phosphorus, and thus "mountain" or magnetic ores which contained considerable percentages were objectionable. There is still, however, in certain parts of America a method in use by which ores containing a considerable quantity of phosphorus can be utilized. This method has been called "metal forging;" but as it is also intermittent, and takes place in open furnaces, it neither properly utilizes the fuel nor returns an equivalent percentage of iron, and has in consequence been found very costly, and therefore is

in use only under exceptional circumstances. It is clear that, if the process of conversion takes place in a shaft, as in a blast-furnace, without the temperature becoming so great as to effect any coalescence or complete smelting, and the mass is, at this stage, transferred in a convenient manner to a hearth where the further process of fusing the iron particles can take place, the process will at once become continuous and direct, and has the advantages of saving fuel and removing any impurities in the bloom at the same time. The furnace, during this operation, can be kept closed, so that reduction by the hot carbonic oxide proceeds continuously. The furnace at Nyhamm consists of a reduction shaft connected with the hearths by small culverts. These hearths can be closed, having vertical dampers with holes at their lower part, in order that the gases generated by the fuel may pass through the shaft and thus act the part of gas in an ordinary blast-furnace. The dampers are balanced, and are therefore easily raised and lowered, the culverts being also furnished with single bricks, by removing which the necessary repairs to the furnace can be done, but which, at other times, close the furnace. Should it be desired to cut off the shaft from the remainder of the furnace, this can be done by a horizontal damper, which can be drawn closely over the hole. The operation of the furnace is as follows: Charcoal and ore are charged in the shaft in proper proportions, either by a special apparatus or in the common way. The ore will then, as it settles in the shaft, be subjected to the same process of conversion as in the ordinary reduction-zone of a blast-furnace. In order to transfer the spongy iron to another hearth, a hook is passed through the upper working holes in the dampers of the culvert through which the operation of raking down is effected in order to keep the hearth always well filled with charcoal and iron until the smelting is nearly effected; but when it is desired to remove the mass of iron, the raking down is stopped, and the bloom allowed to go down in the hearth. It may then be easily broken up when one of the dampers is opened. During this operation one fireplace should be kept charged, as the gas-pressure in the furnace should always be higher than the pressure of air from without, in order to prevent all suction of air through the open hearth.

As soon as the bloom is removed and the hearth cleaned out, it is again closed and refilled with charcoal and iron, by raking down from the shaft as before, and the blast turned on. In the same way, the process may be alternated with the other hearths. The furnace which was erected at Nyhamm consisted of a reduction shaft, 16 feet high, with a cubic diameter of 16 feet above and 18 below, made of fire bricks, and was $1\frac{1}{2}$ feet wide; it contained 302.4 cubic feet charcoal. With this was connected a hearth, the dimensions of which varied, as they were altered considerably during the progress of these experiments. The fittings were made of bar-iron, and were very similar to those used in the Lancashire hearths. The dimensions were as follows: Distance between upper rim of tuyeres, 2 feet; but in order to facilitate the extraction of the bloom, they were made to slope an inch outwards, being thus 2 inches less at the bottom. From the back, which was perpendicular, to the front wall, the distance was 2 feet, with 3 inches slope outwards; but this distance may, perhaps, be somewhat reduced. The depth of the hearth was 1 foot, and the moulds inserted an inch, with a declivity of about 22 degrees, and their width at the nozzle $\frac{7}{8}$ by $\frac{7}{8}$ inch, with the upper sides semicircular.

As only one furnace was erected, it became necessary to have an additional "koltern," or heating apparatus, which was kept going to prevent any suction of air whilst the bloom was removed. In order not to obtain any metal before the tuyeres until the furnace was fully heated, about $9\frac{1}{2}$ cubic feet of charcoal were thrown into the hearth when the bloom had been removed. The front damper was then closed, and charcoal and ore raked down from the shaft till the hearth became nearly filled; the blast was then put on and the raking down continued, according to appearances in the hearth. When the slag made its appearance before the tuyeres, generally half an hour after the blast had been opened, it was tapped in precisely the same manner as in a Lancashire furnace. No particular work in the hearth was required, but when the tuyeres

could not be kept free during the settling, it was found necessary to insert a bar carefully through one of the front dampers in order to ease the mass. This was, however, avoided as much as possible, as the coalescence of the materials was greatly accelerated by any stirring in the hearth, and caused great loss of iron in some instances. The smelting was also imperfectly effected, the bloom being irregular and covered with a slaggy coating. This was particularly the case when the action of the furnace was defective, owing to the choking of the tuyeres by unreduced ores, &c. When the mass commenced to fill the hearth, the slag became more heavy and porous, and poorer in iron; the raking down then ceased. The blast was still continued until the hearth became sufficiently empty to allow the breaking out of the bloom without removing any fuel. Towards the finish some work was done in the hearth with the bar, partly to keep the charcoal over the tuyeres, and partly to fettle up the bloom. This was, however, effected after opening one of the side doors. An advantage which is very considerable as regards the practical utility of this furnace is the great ease with which the raking down is effected, as well as any other operation which may be required in the hearth whilst the blast is on. For instance, when the furnace becomes heated, the flame, which is forced through the holes when these are opened, is so "curt" and transparent that it is quite possible to stand at a distance of 4 to 5 feet from the hearth and look into the furnace whilst raking down charcoal and ores without any inconvenience. With a little practice, which an unskilled laborer may acquire in a week's time, it is possible to charge and rake charcoal and ores uniformly down, an advantage of great importance, as it embodies a check whereby, to a certain extent, the action in the furnace may be kept perfectly even.

The furnace was tended by one man each shift, who, with the assistance of a boy, stored the ores and charcoal and also removed the slag and attended the "koltorn." As the hearth during the process was closed, the flame could only issue from the working-hole through which the furnace was tended; the heat was therefore small, and as the work consisted chiefly of raking down into the hearth, tapping the slag, and keeping the furnace clean, it may be said that the actual labor of tending the furnace was comparatively simple, both as regards the labor involved and the skill required It may be added anybody without experience in tending furnaces can be employed, and one may therefore be entirely independent of the skilled workman, this circumstance being no inconsiderable factor in the method. The shaft was capable of holding from twenty-two to twenty-three charges of two barrels charcoal each, viz., 290 cubic feet each smelting, and one smelting was generally effected during twenty-four hours. In most of the experiments two barrels—12.6 cubic feet of charcoal to 3 cwt. of ore—were used, but towards the finish the quantity of ore was reduced to 2 cwt., *i. e.*, to 1 cwt. per barrel of charcoal (6.3 cubic feet), and this proportion was found advantageous, both as regards ore and the quantity of fuel consumed, in proportion as the ores contain more or less phosphorus. It would, however, be better to keep the slag richer and more plentiful in iron by a greater charging of ore than otherwise, unless it should, of course, be preferred to make the process more basic by a flux of lime or alumina. If such should be the case, it may be pointed out that a flux of this kind would be more effective in effecting dephosphorization than a refining furnace, a result which is brought about by the ferrous oxide contained in the slag appearing to act on the phosphorus in the same manner as lime on sulphur.

The experiments which we record were commenced in November and continued till about the middle of December, and then resumed with few interruptions from January to March. The results arrived at during this period were, of course, variable, as the idea guiding these experiments was to find the best relation between the hearths, their diameter, the number of the tuyeres, their size, inclination, pressure of the blast, &c. We shall, therefore, here only lay before our readers those results which tend to show what might best be effected with such a furnace, the following being the particulars of the working during the last few weeks. The ores used were unroasted iron ores from the Väfspols mine in

Gränges berget, a famous iron deposit in Sweden, and contained about 60 per cent. of iron and 0.91 of phosphorus, which were charged with 1 cwt. of ore per barrel of charcoal, viz., 1 cwt. of ore to 6.3 cubic feet of charcoal.

The results of the following five shifts were:

	Consumption of Charcoal. Barrels.	Ores. Cwt.	Yield of Iron. Cwt.
1 day shift.....	23	20	11.40
1 " "	24½	22	11.60
1 " "	7	6	4.00
1 night shift.....	25	22	10.65
1 day shift.....	21	18	10 80
	100½		
	= 633 cubic ft.	88	48.45

As 12½ of these 100½ barrels were consumed in the fireplace, the actual quantity of fuel used for iron-making was only 88 barrels, or 554.4 cubic feet, for the smelting of some 88 cwt. of ores; the relative consumption being therefore 2.07 barrels, equal 13.04 cubic feet charcoal and 1.80 cwt. ore per cwt. iron returned. The actual returns of iron were thus 55.05 per cent. It ought, however, to be stated that the bloom returned was not weighed separately, but in solid unbroken blocks, and although these when broken up, were found extremely compact and free from slag, the result would, no doubt, not have been so satisfactory had the smeltings been mixed together, just as they came from the hearths. The reason why this was not done was that they were at first too small and loose for the big hammer, and when they became larger and more compact, the Lancashire smiths did not approve of having their materials made impure by these. The only thing to be done was therefore to pile them up till a convenient opportunity arose of having them reheated in the Lancashire hearth, and to this end they were subsequently broken under the crushing hammer, when there was also a good opportunity of examining the fracture, which was generally found somewhat coarse and crystalline, with a finer surface, however, underneath and at the edges, which could, no doubt, be accounted for by the circumstance that these parts had absorbed more carbon.

As a rule three hours were required to smelt a mass of 3 to 4 cwt. It is, therefore, to be expected that the parts which were the longest exposed to contact with the charcoal had absorbed the greatest percentage of carbon; but with increased dimensions of the shaft a more thorough reduction, and therefore an increased production would be effected. The principal work of the furnace would also be to smelt the iron particles effectually, and the mass would not remain so long in the hearth, on one side exposed to carbon cementation, and on the other to the opposite effects of the slag and the blast, thus tending to make the bloom uneven. The effect of these are minimized in proportion, as less time is expended in the smelting, and in consequence a more homogeneous product may be looked for. Owing to the depth of the hearth and the long time which was required for the settling, the bloom became cooled underneath, which made it a work of some difficulty to extract the slag at the notch. This difficulty ought to be avoided, either by heating the mass before it is taken out, or by giving it an appropriate heating in a separate "welding" furnace before breaking it up. Should it be desired to obtain through a resmelting process a thoroughly homogeneous product, this can of course be best effected in a Martin furnace, by which excellent castings may be obtained, even from metal of inferior quality ores, and this charcoal method might therefore become a factor of considerable importance in the Siemens-Martin process. In consequence of the compactness and small caroon contents of the blooms the process of refining the Lancashire furnace was very slow; in fact, there was required as much time as well as fuel to effect the resmelting as to effect an ordinary Lancashire refining. The loss was, therefore, in this case greater than would under other circumstances have been justified; and it should be at once understood that the latter part of the process can never be considered practical or necessary, and it would on the other hand be out of the question with a better regulated working and action, a fact which was fully demonstrated at the crushing of some of the blocks.

The total quantity of iron made was about 300 cwt., from which the following analyses of the contents of phosphorus were made:

		Per Cent. of Phosphorus.
Iron from Björnhytte mine contained		0.02
" "	"	0.06
" Väfpolsgrufvan	"	0.12
" "	"	0.10
" "	"	0.12
" "	"	0.08
" "	"	0.10

The two latter were, however, from blooms which were not resmelted. In the crucible the Väfpols ore yielded 62.3 per cent. of pig iron, with 1.32 per cent. of phosphorus. Three analyses of the iron gave respectively 1.33, 1.48, 1.70 per cent. of phosphorus, equivalent therefore to 3.04, 3.37, 4.10 per cent. phosphoric acid.

Under the tests made on the iron thus manufactured, in order to ascertain its tension, it did not show any tendency to redshortness or brittleness; and by the experiments made at the testing establishment at Liljeholmen on a rolled bar of this iron, 600 lines long and 48 lines in diameter, the limit of elasticity was shown to be = 48 lb. per square line, with a bearing strain=81 lb. per square line with an elongation of 20.8 per cent., a result which, it must be admitted, is very satisfactory, and can compare well with the class of pig-iron made by the Lancashire process. What was fully borne out by the experiments at Nyhamm, and which promises well for the further development of the method as a charcoal reduction process, is the fact that the action in the hearth, and consequently the result, stood in direct proportion to the temperature in the shaft, *i.e.*, to the reduction of the iron before it fills the hearth. If the furnace was sufficiently heated, no hard lumps, for instance, could be noticed chafing the rod when raking down, and the action was then perfectly regular, the moulds were clear, and the formation of slag small; whereas, when this was not the case, the action became at once less satisfactory in proportion as the temperature in the shaft fell. As the temperature in a furnace can be lowered, not only by excessive charging, but also by an action which is either too quick or too slow, &c., the case was just the same in this instance, and the effect analogous, viz., the unreduced metal remains in the slag in the same proportion as the reducing capability of the furnace decreases; and as the iron in the hearth is not overcharged with carbon, besides appearing solid, no boiling could possibly arise from the influence of the iron-charged slag on the bloom; but this circumstance, in addition to the loss occasioned by unreduced iron being absorbed in the slag, should have caused further waste of metal. The question here, therefore, as with all furnaces, is to carefully observe that the charges, their quantity and composition, as well as other circumstances directly affecting the action of the furnace, are all in accord with the object in view, although it may be said that divergences may in the present method not affect the action of this furnace to the extent which is the case with an ordinary blast furnace from the same causes. At the same time it seems from the practical experiences gained from this method that any overcharging of the shaft has an injurious effect on the smelting. We also attach a few particulars of some experiments with the same method made at Söderfos by the candidates at the Royal School of Mines in Sweden. The shaft was in this case 16 feet high, and capable of containing ten charges of two barrels, viz., 12.6 cubic feet each; about half the quantity therefore of the one erected at Nyhamm. The manufacture here was about 17 cwt. pig iron per shift, with a consumption of 25.2 cubic feet charcoal per cwt. pig, and about $\frac{1}{2}$ cwt. ore per barrel charcoal = 6.3 cubic feet. By the experiences thus gained in the method, it seems—whilst, of course, pointing out the improvements and alterations which might be effected for its simplification—that it would be of practical utility as a charcoal process for the direct conversion of ores containing an unusually large amount of phosphorus.

We may, in concluding this article, state that the district of Vesterbergslagen embraces the richest and purest stratum of metalliferous mountain in Sweden, and it is only to be regretted that the quality of the ore is not equal to those generally found in that country. It contains close upon 70 per cent. of pure iron, but as much as 1 to 1.50 per cent. of phosphorus, which, with the means at present at disposal, renders them of little use for the manufacture of steel. The metal from these ores is,

however, largely used for castings, and if the time be not far distant when the charcoal supply of Sweden may fail to satisfy the demand, and coals be required for smelting, the deposit may become a source of immense wealth to that country. Among the extensive iron deposits in this district, the above-mentioned Grangesberg alone contains a bed of iron said to be nearly 15,000 feet long and 1,000 to 1,500 feet wide, consisting partly of peroxide of iron and partly of magnetic iron of volcanic origin; the gangue is quartz and apatite.

A NEW FORM OF VERNIER.

By H. H. LUDLOW, 2d Lieut. 3d Artillery, U.S.A.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

VERNIERS have been so extensively used and brought to such perfection that there seems to be but little room for improvement. There are cases, however, in which a scale very similar in principle is more advantageous.

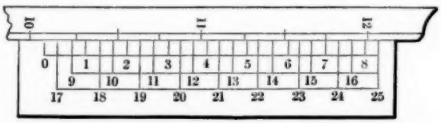
Thus, suppose a main scale divided to $\frac{1}{4}$ in., and an accompanying direct vernier which reads to $\frac{1}{100}$ in., the entire vernier of 25 spaces will exactly cover 24 scale spaces, giving a length of 6 inches.

Instead of the vernier, another scale may be constructed as follows: Let x denote the vernier space expressed in inches. Assume

$$\frac{1}{4}-3x=\frac{1}{100}. \quad . \quad . \quad . \quad . \quad . \quad (1).$$

whence $x=\frac{2}{25}$ inch.

The new vernier or *subscale* is composed of 25 spaces, giving a length of 2 inches. It is represented in the figure,



and is read in the same way as the ordinary vernier. Subscale division numbered 7 is coincident and the reading is 10.07 inches. It gives the same ultimate unit of measure as the above vernier with $\frac{1}{8}$ the length, replacing a vernier of 6 inches by a more convenient one of 2 inches. The numbering of the new vernier is not consecutive. It is as though the vernier first taken had been divided into 3 equal parts which had been superimposed, thereby compressing it to $\frac{1}{3}$ its original size. To obtain an equally convenient vernier of the ordinary form with the same least count, would require a smaller scale space and a greater number of scale divisions.

It might at first sight appear that the coefficient of x in equation (1) may be any whole number; but in fact it must be such that the second number of (1) will exactly divide the value of x, otherwise this second number will not be the least count. For example, suppose

$$\frac{1}{4}-5x=\frac{1}{100}. \quad . \quad . \quad . \quad . \quad . \quad (2).$$

$x=\frac{24}{500}=\frac{6}{125}$. Then $\frac{5}{4}-26x=\frac{1}{500}$, showing that 26 subscale spaces differ by $\frac{1}{500}$ inch from 5 scale spaces. The corresponding subscale is direct, has a least count of $\frac{1}{500}$ inch, contains 125 subscale spaces, and exactly covers 6 scale spaces, giving an entire length of $1\frac{1}{2}$ inches. It would have its divisions too close together to be seen distinctly without a magnifier, and would not be convenient in use. The divisions would be numbered with intervals of 26 subscale spaces between consecutive numbers instead of 3, as in the figure.

The subject is worthy of careful consideration by those interested in devices for accurate measurement. In favorable cases the new form procures accuracy and convenience with a less number of scale divisions, thereby diminishing the cost of the entire instrument.

THE EDISON ELECTRIC LIGHT METER.*

By FRANCIS JEHL.

The principle upon which this meter is founded is known as electro-metallurgy, that is, the disruption or tearing away of a metal by electricity, from one electrode and its deposition upon the opposite.

FUNDAMENTAL PRINCIPLES.

If an electric current, no matter how generated, whether by a dynamo machine, or voltaic element, be made to pass by means of platinum electrodes through acidulated water, electrolysis takes place, that is, the current has the power of loosening and separating certain chemical compounds—in other words, it decomposes the compound through which it has passed. Any substance which is susceptible of decomposition by an electric current is termed an "electrolyte."

By the term electrodes is always understood the two extremities or poles which lead from a source of electricity.

Electrodes are divided into anodes and cathodes.

The positive electrode is called the anode, and the negative the cathode.

The products of decomposition, or the substances which gather at each pole during electrolysis, are termed "ions." That which gathers at the anode is called anion, and that which gathers at the cathode is called cation.

The amount of current required for decomposition varies greatly with different electrolytes.

In the above mentioned case, where the current passes through acidulated water, oxygen gas is liberated at the anode, and hydrogen at the cathode.

If into this liquid which contains the acid some crystals of sulphate of copper ($CuSo_4$) be thrown, electrolytic action will still continue, but in a different manner, oxygen will be evolved, and copper will be deposited on one of the platinum electrodes, while the hydrogen takes the place of the copper in the solution. It may be represented chemically by $H_2O + CuSO_4$ before the current has passed, and $O + Cu + H_2SO_4$ after the current has passed.

If in the above experiment, a copper electrode be substituted for the positive, it will be found that no gas will be liberated, the hydrogen, as before, will take the place of the copper in the solution—the oxygen, instead of escaping at the anode, will combine with the copper of the electrode and the sulphuric acid, to form sulphate of copper.

The chemical forces, called into action by the current, are so beautifully balanced, that in our last experiment the quantity of copper, supplied by the positive electrode, exactly equals the quantity withdrawn from our solution and deposited upon the negative electrode.

LAWS OF ELECTROLYSIS.

The following were demonstrated and discovered by Faraday.

Electrolysis cannot take place unless the electrolyte is a conductor.

The energy of the electrolytic action of the current is the same in all parts.

The same quantity of electricity—that is, the same electric current—decomposes chemically equivalent quantities of all the bodies which it traverses; from which it follows that the weights of the elements separated into these electrolytes are to each other as their chemical equivalents. For instance, in the decomposition of water it will be found that for every 18 parts of water decomposed two parts will be hydrogen and 16 oxygen; in order to form water from its two component gases we must take them in the above ratio.

It also follows from the preceding law that the quantity of the substance which is decomposed is proportional to the total quantity of electricity which passed through it, and is independent of the time during which the electricity passed; the quantity corresponding to the passage of one unit is called the electro-

* Under the above title this article was originally published in London in pamphlet form. For presentation to the scientific public such parts of the original as pertained to the manipulation of the meter have been omitted, but the complete exposition of the principles upon which it operates are retained.

chemical equivalent of the substance. Thus, when one unit of electricity passes through a solution of sulphate of zinc, having platinum electrodes, one electro-chemical equivalent of zinc appears at the cathode, and one electro-chemical equivalent of oxygen at the anode, while one electro-chemical equivalent of sulphate of zinc has disappeared from the solution, but an equivalent of sulphuric acid has taken its place. If, in the above experiment, zinc electrodes were used, the action would be as follows:

For one unit of electricity, one electro-chemical equivalent of zinc would appear at the cathode, one electro-chemical equivalent of oxygen at the anode, there uniting with the zinc and sulphuric acid to form another electro-chemical equivalent of sulphate of zinc, and taking the place of the one just decomposed. This action continues, and keeps on depositing zinc on the cathode, and taking zinc off at the anode.

Upon the preceding law has Mr. Edison based his meter, and no matter how much current passes through it, for every electrical unit or fraction (which unit is called an Ampère), there will be a corresponding number of units or fraction of a unit of the metal deposited.

POLARIZATION.

If, in a circuit consisting of an electrolytic cell containing acidulated water, having platinum plates for electrodes, we insert a single voltaic element together with a galvanometer to measure the current, we find that the strength of the current rapidly diminishes on closing the circuit.

Neither oxygen nor hydrogen appears in a gaseous form at the electrodes, but the electrodes have acquired new properties, showing that a chemical action has taken place at the surface of the plates. If now the battery be disconnected, and the galvanometer alone, with the electrolytic cell, remains in the circuit, it will be found on closing it that a current is traversing, and showing on the galvanometer that it is in an opposite direction to the original current. This current rapidly diminishes in strength and soon vanishes. It can also be seen that this current is not as strong as the primitive one. This acquirement of the electrodes is termed polarization.

In the construction of an electric meter, such elements must be used as will not, under any circumstances, polarize; for suppose an electrolytic cell, which was capable of being polarized was used to ascertain the amount of current that was passing through the line in which it was inserted it would, in the first place, have the tendency to weaken the original current, and, if the instrument was shunted, as is essential in electric lighting, this counter current would all the while resist the original current, causing an erroneous deposit, it depositing less metal than would be deposited if there were no polarization. Then, again, when the current on the line ceases to flow, this counter current would begin to act and redeposit some of the metal which the original current had deposited. Thus we see why any elements capable of polarization would not do for an accurate meter. Then again, there is another consideration that comes into play, and that is, that nearly all elements when immersed in a solution, generate a small current, for example: Two plates of copper in a solution of sulphate of copper, when connected with a galvanometer, will indicate the presence of a current. Now, in the above case, when the electrolytic cell was shunted it had necessarily, a closed circuit. The circuit being closed, this current, as indicated by the galvanometer in the last experiment, would become active, and deposit metal while there was no current circulating in the line. This current, although feeble, will in time deposit a considerable amount of copper, and cause an inaccuracy almost inconceivable. A copper deposition cell, and some other metals, is suitable for large currents, and when the plates are taken out of the solution, immediately after the current ceases to flow; but when it is required to register a very small current, such as $\frac{1}{1000}$ of an Ampère, and when the deposition cell is always on a closed circuit, it becomes necessary to use some other metal than copper in order to obtain accurate results.

In order to get rid of this difficulty of polarization, Mr. Edison found that by using electrodes of pure zinc, amalgamated with pure mercury and a solution

of chemically pure sulphate of zinc, that there is almost no polarization, and great practical accuracy is insured when an exceedingly small quantity of current is desired to be measured. The same is true if the currents be of large dimensions.

I may add that it is advisable in all electrical researches, whenever it becomes necessary to ascertain the magnitudes of an unknown current, and especially if it be small, that instead of using the copper deposition method an electrolytic ele ment consisting of pure zincs amalgamated with pure mercury in a chemically pure solution of sulphate of zinc be used.

RESISTANCE OF ELECTROLYTES AND METALS.

It is very difficult to measure the electric resistance of some electrolytes on account of the polarization of the electrodes. In order to overcome this difficulty one must use, as stated in the preceding article, zinc electrodes. There are other methods for ascertaining the resistance of solutions, but it is not necessary for me here to explain such methods. The temperature of the solution greatly affects its resistance. It will be found that its resistance decreases as the temperature increases, or when the temperature decreases the resistance increases. Thus we see it has properties similar to carbon, for carbon will decrease its resistance when its temperature is increased and *vice versa*. These properties are just the reverse of those exhibited by the metals.

We, therefore, lay down the following laws, namely:

That the resistance of electrolytes diminish as the temperature increases.

The resistance of metals increases as the temperature increases.

Now, it is obvious that, if we ascertain the resistance of a certain solution at different temperatures, we can ascertain the difference of its resistance between such temperatures. For example, if a solution of sulphate of zinc at 0° C., and specific gravity 1.29, offers a resistance of 1.40 ohms, at a temperature of 50° C. its resistance is diminished to 0.32 ohms. Therefore, the difference between those two temperatures is—

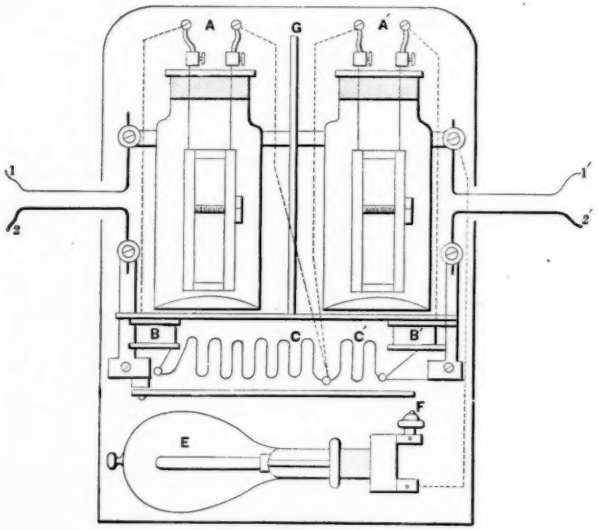
0°	1.40
50°	0.32
	1.08

showing a decrease of 1.08 ohms between the limits of 0° and 50° C. If we remember that this difference is in contrary direction to that of copper, it will be seen that if we take a certain length of copper wire which changes its resistance between 0° and 50° by the same amount as the solution but in the opposite direction, that by placing the two in series, that is in the same circuit with each other, one would compensate for the other, that while one diminishes the other increases, and the circuit in which they are placed maintains a constant resistance and does not vary with the temperature. Mr. Edison has made use of these principles in his meter, and has a constant resistance in the circuit where the deposition cells are placed, without which an electric meter would be of no value where there is a change of temperature.

GENERAL DESCRIPTION OF THE METER.

The meter is divided into two compartments. The first, or the one on the left side, is termed the monthly cell. This is taken out every month by some employee of the company, and another cell is substituted for it. The one taken out is returned to the station, where the plate that has received the deposition is weighed. The cell on the right hand of the meter is termed the quarter yearly cell, and is a check cell. The party that has access to the monthly cell has not to the quarter yearly cell. This quarter yearly cell is taken out every three months and the deposit weighed. Its deposit must bear a certain proportion to the sum of the monthly meter deposit for those three months. If its deposit does not agree in proportion to the monthly cell, there is something wrong, or somebody has tampered with it. Thus we see the object of this auxiliary cell. In the diagram A is the monthly cell, and A′ is the quarter yearly cell. B′ and B are the compensating resistances, in series with the cells A′ and A respectively, the object of which has been explained.

C and C′ are the respective shunts, (made of bands of German silver), from which the cells A and A′ receive their current. In all meters, irrespective of their capacity for registering, the resistance of the cell A with its compensating resistance B is 830 times the resistance of the shunt C, and the resistance of A′ plus B′ (equal to A plus B) is 3320 times the resistance of C′ within the ranges of temperature occurring in practice. The resistance of C′ is therefore one-fourth that of C and the cell A′ will receive one-fourth as much deposit as A.



D is a thermo arrangement which prevents the freezing of the sulphate of zinc solution in the winter, or too low a temperature for accurate registration. It consists of a strip of brass and steel riveted together.

The unequal expansion and contraction of the two metals causes contact to be made at F when the temperature falls to the lowest desired limit to which F is adjusted. This throws the lamp E in circuit, the heat from which raises the temperature in the meter, and acts upon the thermo strip causing it to open the lamp circuit.

METHOD OF CALCULATION.

Whenever a meter is set up to register the consumption at any place the weight of the plates is recorded. The weight of the plates from the monthly cell is taken after they have been in use the required length of time, and upon the gain in weight of one plate is based the amount of current used. The gain in the quarter-yearly cell should, in the given time, equal one-fourth the gain in the monthly cell for three months.

It has been stated that the resistance of the circuit containing the monthly cell is 830 times the resistance of the shunt around which it is placed, therefore of the total current passing $\frac{1}{831}$st part will pass through the cell and be registered.

If it is experimentally determined that the $\frac{1}{831}$st part of an Ampère flowing through the cell for one hour will deposit 1.6 milligrammes, then is this the true indication of one Ampère for one hour, because the remaining $\frac{830}{831}$ will flow around the cell and through the shunt without being registered.

To find the number of Ampères for one hour, therefore,

$\frac{\text{Gain in milligrammes}}{1.6}$ = Ampères flowing for one hour.

This result may also be expressed as "number of hours for one Ampère."

If one lamp, giving 16 candle-power of light, requires a current of three-fourths of an Ampère, the amount of deposit in the cell in one hour for this lamp would be three-fourths of 1.6 milligrammes = 1.2 milligrammes.

Therefore, to find the number of lamps operating for one hour to produce the deposit,

$\frac{\text{Gain in milligrammes}}{1.2}$ = number lamps for one hour.

This result may also be expressed as "number of hours for one lamp."

Thus the gain of weight in one plate bears a constant ratio to the current which has passed through under a uniform pressure, and also to the energy consumed beyond the meter, and is therefore a register of the amount of energy, irrespective of the particular use to which it was applied.

ON VARIATIONS IN THE LIMIT OF ELASTICITY AND IN THE MODULUS OF ELASTICITY OF VARIOUS METALS.

By PROF. J. BAUSCHINGER.

From "Der Civilingenieur," for Abstracts of the Institution of Civil Engineers.

The paper contains numerous and extensive tables, the results of various experiments made on bars of weld-iron,* ingot-iron, Bessemer steel and bronze, and on plates of copper. In Dingler's Journal, vol. 224, the author proved the following law to hold in the case of Bessemer steel:—"By stretching the metal beyond its elastic limit, the range of the elasticity is increased not only during the time for which the load is applied, but also for a considerable period of repose after the stretching (*i. e.* while the bar is unloaded); such period extending to one or several days, and by this means the elastic limit itself can be raised to a limit greater than the load which caused the original extension." There can scarcely be a doubt that this is due to the effect of what has long been known as "secondary elastic action," and it agrees with some results obtained by Wöhler, and published in Erbkam's *Zeitschrift fur Bauwesen* as far back as 1863. In continuing his experiments on the subject, the author proposed to himself two questions:—

1. What influence the length of the period of repose, following the extension of the bar under a given load, had upon the magnitude of the consequent increase of the elastic limit? and, 2. Whether any and what alteration of the modulus of elasticity was thereby brought about?

The testing machine used permitted of variations in the length of the bars to be read to the ten-thousandth of a millimeter; and since the parts of the bars tested were all originally 15 centimeters long, this corresponds to the variation of the 1,500,000th part of the length, and the author claims that the use of such delicate measurements must lead to a clearing up of the views held as to the limit of elasticity, the definition of which became uncertain as soon as it was known that permanent alterations of length were produced by relatively light loads.

Measurements with the apparatus used, show that in materials known to be elastic, such as wrought iron, steel, wood, &c., Hooke's old law "*ut tensio sic vis*" (*i.e.*, the proportionality of the alteration of length to the load which produces it), always holds strictly within a certain limited range. Once this range—which the author proposes to call the limit of proportionality—is passed, the extensions become gradually greater and greater under successive equal increments of load. With many materials, especially

* This nomenclature is used as an attempt to follow out the classification of wrought iron recently proposed by Prof. Bauschinger, and to some extent adopted in Germany.

weld and ingot iron and the softer kinds of steel, a second noteworthy point is reached by a gradual increase of the load above the limit of proportionality. The extension is gradual under successive equal increments of load, until this point is reached, but then suddenly becomes very rapid—so rapid that the image of the scale on which the extension is measured passes out of the field of view of the telescope, so that a reading is no longer possible. Under a greater load than that corresponding to this limit, the scale does not come back into the field of the telescope till after a long interval, of at least several hours, of quietude; *i.e.*, the secondary elastic effort has to act for several hours, and in some cases with high loads for several days, in diminution of the effect produced at once by application of the load. This point may be called the "drawing-out-limit,"* and the analogous point in case of compression the "bulging-limit." The total effect may be exhibited graphically thus: If the successive loads are set off as abscissæ along any line, and corresponding ordinates are drawn proportional in length to the extensions caused by the loads, a curve drawn through the extremities of the ordinates may be called the stress curve. Within the limit of proportionality this curve will be (at least approximately) a straight line, but beyond it will gradually become more and more curved while at the "drawing-out limit" the curve will show a more or less sharply defined bend or angle.

The author discusses the two following recently proposed definitions of the limit of elasticity. One by Wertheim: the stress under which the permanent extension caused by it amounts to the twenty-thousandth part of the original length; the other by Styffe: if a bar of iron or steel is gradually stretched under a series of loads, the first being so small as to cause no permanent set, each acting for the same number of minutes, and which are so increased that each increment is the same percentage of the whole load, then the elastic limit is the stress under which, acting for the prescribed time, there is a permanent extension bearing to the length of the bar the ratio of 0.01 of the ratio of the increase of weight to the whole load. Under Wertheim's definition the permissible extension in an original length of 15 centimeters would be 0.0075 millimeter, but the tabular results of the experiments show that, with ordinary materials, the limit of proportionality is generally passed long before this extension is reached, and frequently the "drawing-out limit" also, when it exists. In Styffe's definition time is made an element, which in the author's view should not be, and he shows by an example that the definition may lead to a stress being taken far above the "limit of proportionality," and maintains in consequence that such arbitrary definitions are inadmissible, and that the limit of elasticity ought to be the "limit of proportionality," as tested for each particular material. A consequence of this would be that materials such as cast iron and stone would simply haveno elastic limit.

The time which is allowed to intervene between successive loadings of the bar appears to have considerable influence on its behavior: as the following figures selected from the tables will show:—

A. A Bar of Weld-Iron.

	Kilograms per square centimeter. (0.00635 ton per square inch.)			
	Limit of elasticity.	Stretch limit.	Load removed.	Mean modulus of elasticity.
First time of testing	1,414	1,919	2,222	2,060,000
Second testing, immediately following first	1,010	2,222	2,828	1,964,000
Third testing, immediately following second	1,048	2,935	3,354	1,946,000
Fourth testing, immediately following third	1,087	3,478	—	1,937,000

*It appears to correspond with what Prof. Kennedy has called the "breaking-down limit."

The maximum stress produced a permanent extension of 41 millionths of the original length.

B. A PRECISELY SIMILAR BAR.

	Kilograms per square centimeter. (0.00635 ton per square inch.)			
	Limit of elasticity.	Stretch limit.	Load removed.	Mean modulus of elasticity.
First time of testing	1,610	2,113	2,213	2,060,000
Second testing, eighty hours after the first	2,240	2,444	2,851	2,026,000
Third testing, sixty-eight hours after the second	2,485	3,106	3,313	1,985,000
Fourth testing, sixty-four hours after the third	2,982	—	3,408	2,018,000

And under this maximum stress the elongation was 18 millionths of the original length.

From the series A it will be seen that, when no appreciable interval occurs between the loadings, the limit of elasticity is diminished at the second application, but afterwards gradually increases, and that the "drawing out limit" increases throughout. From the series B it will be seen that, with considerable intervals of repose, both limits are steadily raised throughout. This appears to hold generally.

ON A NEW SYSTEM OF HYDRAULIC PROPULSION.

By VICE-ADMIRAL J. H. SELWYN.

From the "Journal of the Royal United Service Institution."

THE subject to which I am about to draw your attention is one of considerable interest, not only on account of its connection with hydraulic propulsion, but as leading to the study of a hitherto neglected branch of hydrodynamics, which may even influence, when thoroughly understood, some of the accepted physical theories.

We are all more or less familiar with the various forms in which machines for utilizing water-power have been made. In useful effect produced, no doubt the turbine stands at the head of the list, and the attempts hitherto made to apply hydraulic propulsion to vessels have almost invariably comprised some form of turbine, to which the power of the engine was applied, in order to obtain a reactive effect from water set in motion. But in every one of these systems, not excluding the most modern form of centrifugal pump, the methods employed were such as to produce the following effects:

First, the water was set in motion by discs, fans, vanes, paddles, or screws, inside a casing, which confined it, so as to produce a pressure.

Next, the water under such pressure was caused to take a determinate direction.

Lastly, a controllable ultimate direction was imparted to the water, which might be forward, backward, or opposite on the two sides, or, again, entirely annulled by being converted into upward pressure, at the will of the operator, and without interfering with the movement of the engines.

It was, in fact, the realization of a most perfect form of propulsion, which, being entirely based on reactive effect, was not, and could not be, dependent, like the paddle and screw, on the steadiness of the vessel for its maximum useful effect, besides presenting many other advantages which have been often brought to the notice of this Institution, and which it would be out of place to bring forward again on this occasion.

But there have been also objections made to the use of hydraulic propulsion, and these have been invariably on the score of lower speed obtained with a given I.H.P., since nothing else could have been adduced against a system which on all other points showed so unmistakable a superiority. No impartial observer will allow, if he is fully in possession of the facts, that any such defect in speed has been shown, but the objection still has great weight with large numbers of persons, who ought to be better informed on a matter so nearly affecting the maritime interests of Great Britain.

But we will, if you please, for a moment consider what the objection would amount to, were it absolutely true. More I.H.P., and therefore more fuel, must be used; but this would be all, and with more economical modes of burning fuel and less of the "baseless superstitions of the profession" (as a great American engineering authority has called them) as to the pressure at which we use steam, this increase of fuel expenditure might be nullified. Would this be the case with the paddle and the screw? Clearly the question must be answered in the negative, for both being dependent on the area of water against which they push for their reactive effect, and this area being limited constantly by the draught of water of the vessel to which they are applied, and occasionally by her movements in pitching and rolling, can never be equally efficient with the internal reactive effect produced by a properly constructed hydraulic propeller. The problem involved in the construction of such an instrument is much more complex than would at first sight appear probable, and we shall find that one of the first conditions of success is, that all change in the direction of the water when set in motion by the machine, which is not necessary for our purposes, is to be sedulously avoided. Next, that all lifting of a column of water detracts from the propulsive effect, since whatever power is absorbed for this purpose is taken from that which is available for setting the water in motion in a direction contrary to the path of the vessel, and it is from this source that we expect our forward motion. Thus the water ought to be taken into the vessel when moving with the least possible effort, and leave it with the least possible shock.

Theoretically, therefore, the water should enter the bottom of the vessel by its own gravity, should ascend an inclined tube forming part of the vertically disposed propeller casing, and having had motion imparted to it by the propeller, should leave the vessel immediately above water, with the velocity and area necessary to overcome the resistance of the vessel, and to give her the desired speed. But there should be no whirling or vortex action of the water, and no changes of cross-sections or bends in the tube, since all these tend to diminish the ultimate velocity with which the water leaves the vessel, and v being velocity in feet per second, pressure in pounds on the square foot is $v^2 \times .976$, but little less than the square of the velocity itself.

In the "Waterwitch" I find—

Area of orifices of discharge, 6 square feet,

Velocity of water of discharge, 30 feet per second,

and by the foregoing formula, 878.4 lbs. per square foot, which gives for 6 square feet 5,268 lbs.

Now it may fairly be said that all those hydraulic propellers we have hitherto seen applied, have the features, which I have referred to as being theoretically objectionable, very strongly developed. They do interrupt the motion, they do create vortices, and they have contractions and bends in the channels of the water. They also develop a pressure in the casing, due to these circumstances, which, though it may be, nay is, indispensable in a pump or a revolution indicator like Mr. Tower's, is positively to be avoided in a propeller.

Yet, in spite of all these defects, the hydraulic propeller has given a speed of vessel equal to that of the screw, under, as nearly as possible, similar conditions.

It is also to be remarked, that it has never yet been tried under those conditions of high velocity which would be most favorable to its action and most fatal to that of the screw, unless we are to admit unlimited draught of water or a reduplication, which I should consider most objectionable, if the effect we seek can be got without it.

Having thus glanced at the merits and defects of known systems of propulsion, I propose to bring before you the invention of Mr. George Wilson, C.E., who is the author of papers on the "Flow of Gaseous Substances into each other at High Pressures," and who has, in Holland, had extensive experience in the use of Gwynne's and other centrifugal pumps.

I said at the commencement that I was about to refer to a neglected branch of hydrodynamics. It is this: That water (indeed every fluid or gaseous body) adheres to solids with a force proportioned to the square of the velocity with which the solid passes through it. Now, there are many familiar instances in which this effect is seen. If a grindstone be driven fast in a trough filled with water, not only is the water centrifugally dispersed, but a film of water will be seen ascending higher and growing thicker on the periphery as the speed is increased. If a fly wheel pit be filled with water the rim of the wheel, though turned smooth, and more, the smoother it is, will instantly do as the grindstone did. If, again, a circular saw be drowned in water, it will empty its own pit. A ship also carries, as we know, a skin of water with her. Neither has the principle been left without its application in pumps, for Messrs. Gwynne's pumps have been most successful since the internal wheel took the shape of a disc, on which the blades of the former turbine remain only as mere adjuncts. In propellers, too, Mr. Aston's paddle-wheels, which had no paddles, but only rims, are an application of the same principle.

But none of these are capable of perfectly fulfilling the conditions which ought to be obtained for the propulsion of vessels with convenience and economy, the rim paddle because of the position and size, the centrifugal pump because it creates a vortex, and all modifications of paddles revolving in cases because they create counter-currents which impede instead of assisting the motion of the water in a determinate direction.

You will, perhaps, be surprised to hear that a common grooved pulley, differing from the sheave of a block only in size and shape of groove, has been found capable of doing what is wanted without any of these impediments, and that the smoother the pulley, the better the effect produced.

The size of pulley, or diameter, is dependent upon the circumstances of the particular vessel that has to be moved, and the velocity with which it is sought to move her; but it may generally be said, that in light draught vessels a small wheel with a high velocity will be found most convenient, and in deep draught vessels a large wheel with less speed of piston; and this suits well with other requirements, since, while we have been able to drive small engines at very high speeds, it is difficult, *with any reciprocating system of engine*, to obtain high velocity without serious strains, when great weights are employed.

To give some practical idea of the machine proposed, we will take two types of vessel, one of light, the other of deep draught, and show the calculation. "A" is a vessel whose draught of water is 4 feet, her mid section 80 square feet, and her wetted surface 2,000 square feet.

The diameter of each of two pulleys, applied on the main shaft of engine (which is fixed transversely, and has a speed of 300 revolutions per minute), is 4 feet 6 inches, therefore roughly the circumference is 13 feet 6 inches. This pulley is 30 inches wide, and has in it a parabolic groove 15 inches deep. Half of this depth has to be deducted to arrive at the mean active periphery. The pulley will therefore be calculated as being 3 feet 3 inches in diameter, and 9.9 in circumference: $9.75 \times 300 = 2{,}925$ feet per minute, about 48 feet per second.

The "Waterwitch" attained a speed of 9 knots or 15.21 feet per second, with a velocity of 30 feet per second, and the effect is known to increase as the square of the velocity, so that if our area is sufficient we ought to get with 48 feet per second a speed of ship of about 14 knots, unless the resistance due to form is greater than in the "Waterwitch." Now, let us see what area we have, and how many pounds pressure on that area.

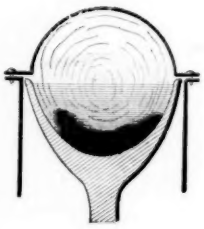
The area of the parabola is two-thirds of that of an equal square. We have here 30 inches $\times 15 = 450$, two-thirds of which is 300: area is therefore 300 square inches. As before $v^2 \times .976$ is

pounds pressure per square foot, and amounts to 2,247 lbs., which multiplied by 2, the square feet in area, gives 4,494 lbs. as the pressure exerted at each pulley (roughly about 2 tons). We know that with the paddle and screw, from numerous independent experiments and experimenters, the tractive force due to 100 I.H.P. is about 2 tons.

We also know that .301 of an I.H.P. per square foot of wetted surface will drive an ordinary ironclad 15 knots with twin screw. Further, that 3 I.H.P. per square foot of mid section is a fair allowance for 12 knots. I might say a very full allowance if it be effective horse-power. With these data it becomes easy to calculate what horse-power the engines should exert to drive such a vessel at any given speed, remembering always that with such an instrument as this all increase of power in the engines will constantly be felt as increase of propulsive effort, in the proportion of the squares of the increased velocity.

We will now take the calculation for the deep draught ship, say 22 feet draught, with the usual proportions for a fast vessel in other respects, but limiting ourselves to 70 revolutions of the engines, and a single engine, not two or more, which might evidently be used if preferred. "B," then, will have two pulleys, or wheels, on each side, of which the external diameter will be 20 feet, the groove 3 feet wide, and the depth of groove 18 inches, with 70 revolutions, the velocity will be 59 feet per second, and the speed of ship about 17 knots, if there be sufficient area. The area will be 864 square inches, and the pressure per square foot 3,397 lbs. $\times$ 6 = 20,382 lbs. on each of the two jets. But 20,382 lbs. is only equal to a little over 9 tons, and as with such a ship we should employ about 3,000 I.H.P., each hundred of which would give a pull of 2 tons, or 60 in all, it is clear that the above area will be entirely insufficient for our purpose. We want at least three times as much, or six such pulleys on each side. That is about 18 feet of pulleys in the thickness on each side of the engine, which would be absurd. Now, suppose we can increase the number of revolutions of the engine to 140 without difficulty, and I am disposed to think this might be done, what help should we get in that direction? The velocity would rise to 118 feet per second, and 118^2 gives 13,924, say 6 tons per square foot. Now we have 6 square feet in each jet and 6 tons pressure per square foot, so we should have 72 tons pressure in all, or more than we require as the result of 3,000 I.H.P. So that there is no insuperable difficulty in the application even in what must be regarded as an extreme case, for if the engines were duplex, as in twin screws, it would be easier to attain the results, and there would be some other advantages gained in the event of one engine breaking down, or where rapid turning power was required.

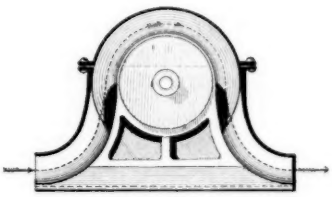
It is also possible to increase the area of groove by making the casing which must always surround the pulleys in a parabolic or circular form, so that the cross-section of any part of the groove will be parabolic in the groove and semi-



circular in the casing, and this will very likely be found to be the most perfect form, particularly at very high velocities, where the water may almost be considered as a rope passing through the vessel, by which she is dragged along, much as a railway engine drags itself and its load along a rail.

Hitherto, I have only spoken of the pulley or wheel, but you will see by the models and drawings that there is another very important feature. The water only enters on the wheel and leaves it at the semi-diameter, because this is the limit of the useful motion that can be imparted or communicated. All beyond the semi-diameter, whether the water be conducted over or under the wheel, though useful in a pump, would be dead loss in a propeller. To meet this condition there is introduced a species of diaphragm of peculiar shape and section

fitting nearly the lower part of the groove, and having curved surfaces, which form a continuation of the limits of what has been called the "rope of water," which form in fact with the casing a pipe through which that rope of water passes. It will easily be seen that the tendency of water set in motion by any portion of the periphery of a wheel, and prevented from flying off centrifugally, would be to follow the periphery in its circular path, as in the helical pump, the disc pump, and all centrifugal pumps pure and simple. But with the condition of propulsion to fulfil, the energy must be directed in another path, namely, that which is opposite to the progress of the vessel, and in this machine it is done by, so to speak, scraping the water off the wheel, and diverting its motion into the needed curve. In doing this, there must necessarily be a slight loss of power, but it is the least possible, consistently with the effect to be produced. The path of the water is shown by the arrows and dotted line in No. 2 diagram. Arrangements are made by



which the reversal or interruption of the motion can be effected, while the engines continue to exert their full speed ahead something in the same way as in the "Waterwitch."

I have now put before you the shape of the instrument proposed and given you some account of the way in which it does its work theoretically. But this latter would be incomplete, were we not to examine the question of hydrodynamics involved. In Mr. Scott Russell's paper (vol. xxii, No. CII of the Journal of this Institution), are some statements which show very clearly how water is acted on by wind. Here is a case, not of a solid body imparting motion to water confined in a casing, but of water set in motion by the impact of the atmosphere, the direct pressure of which is, according to Mr. Scott Russell:

1 lb.	for wind at	20	miles	per hour.
4 lbs.	"	40	"	"
9 lbs.	"	60	"	"

Query, what is the pressure to be added on account of direct weight of atmosphere.

He says also, that 4 lbs., or the sixteenth part of the weight of a cubic foot of salt water, could communicate a velocity of 2 feet per second to 1 foot of water in one second of time.

These statements will serve to show what we might expect from such a pulley as I have been describing, set in motion at such a speed in a body of water. From another paper on *Tower's Revolution Indicator* (vol. xxiv, No. CV), we find that in that instrument, which is a paddle turbine, raising water in a confined column to a height corresponding to the number of revolutions, the elevation of the column is precisely that due to the number of rotations multiplied by the *external*, not the *mean* circumference of the wheel, and calculated according to the laws of falling bodies. Therefore, even at the comparatively slow speed of 60 or 70 revolutions, we might be sure that the whole of the water is really set in motion, since the atoms must re-act on each other precisely as they do when wave motion is produced by wind, with the remarkable difference, however, that the motion is propagated from the motor outwards, not from the surface inwards, and thus in some measure resembles the wave of translation, which delivers its force through any distance without diminution. It is now necessary that I should tell you what has actually been done in practice. Engineers of high standing had predicted utter failure. They said that it was absurd to suppose that a smooth pulley could communicate any motion to water. It ought at least to be roughened, if it did not require paddles; this was disproved *in a bucket*. Then "it might move water in that way, but it could never act as a pump;" this was disproved in a tank. Then it could, at least, never answer as a propeller; this has been disproved in a boat. I have not the least doubt that it will now pass into

the second phase of inventions. The first is, "the thing is not good;" the second is, "the thing is not new." After these are disposed of there will, no doubt, come some other phases of the subject, which are principally disguised attempts to appropriate the profits; and I can only say, though I have no other than a scientific interest in the question, that I hope the inventor will get his reward in due time, and not be left to languish like "Screw" Smith, and so many others of our cleverest inventors. At the beginning of this paper I spoke of the subject being an interesting one from purely physical points of view, and I wish briefly to call your attention to this part of the subject. If we admit that the adhesion of water to a solid moving in it is so great that the whole velocity of the moving body can be imparted to it, we shall first see the importance of skin friction in ships, and be able, perhaps, to measure it more accurately. We shall be able to find out the value of the same force acting on the surface of our screws; we shall be led to reconsider the whole problem of pumping engines at high speeds (the account of the work done by a centrifugal pump at Crossness shows the necessity of this), and generally there will be a new light thrown on many most interesting problems in hydrodynamics.

But we may go even farther, I conceive, and examine into the great forces at work on the globe, either to retain the water of the ocean in its place against the centrifugal force, or to cause the motion of great bodies of water from east to west. What may not be due to a speed of a quarter of a mile per second, if with the petty speed of under 100 feet per second, such results in propulsion may be produced. I venture to commend the whole subject to the younger members of the naval profession as one full of interest for them, but there is matter enough for thought in it for engineers and philosophers of the very highest caliber, and by these I hope it will be taken up and thoroughly investigated. I believe we shall find a law prevailing that speed of rotation being a quarter of a mile per second, adhesion is absolute. Finally, I have only to say that when a vessel of about 130 tons now preparing is completed, I shall be happy to give a more complete account of the advantages of this mode of propulsion, combined with the Perkins engines of 200 I.H.P. This I hope to be able to do some time in the autumn of this year.

CONCRETE SEWERS ABROAD.

From "The Builder."

The construction of concrete drains is increasing yearly on the Continent, notwithstanding the competition of earthenware pipes. These drains are made in two ways. Either concrete pipes or drain pieces are joined by concrete mortar, or the mould of the drain is put up on the spot, and concrete rammed round it into the soil. Although the latter mode of proceeding is the cheapest, and possesses beside the advantage of homogeneousness and better conditions of drying, the erection of the mould, and especially obtaining an accurate angle of fall and small gradients, offers no slight difficulty. After removing the mould or centering, moreover, the inside requires attention, if the whole is to be finished off carefully.

These difficulties have induced Herr J. Chailly, of Vienna, manufacturer of concrete goods, who distinguished himself as a member of the Austrian committee appointed to fix a concrete standard, to construct centering for concrete sewers by means of which the desired form of section, and the inner surface of the drain, may be made so exactly and smoothly as to dispense with subsequent finishing off. The saving thus effected is said to be the least advantage, the principal one being that the sewer may be constructed with a degree of almost mathematical exactness, which insures a

rapid draining off of fluids and prevents accumulation injurious to health. The apparatus recommends itself also on account of its cheapness, a length of only 6 feet being required; as soon as that length of drain is completed, the apparatus is withdrawn, and a fresh piece begun. The time taken in completing a length is three hours, so that in a working day of twelve hours about 25 feet may be made. The concrete being rammed into the soil, and thus becoming closely connected with it, settlements and cracks are out of the question. It is claimed for the apparatus that, the mould being firmly fixed, it does not move even during the operation of ramming the concrete, while with other systems it is shaken about, and it is impossible to maintain the same direction and an exact level. After the piece of drain is finished, the apparatus may be loosened easily and without friction, and moved forward. A number of concrete sewers have been made with Chailly's apparatus; for instance, 20,000 feet run at Linz, as well as many drains at Vienna, Teschen, &c.

The construction of the apparatus is as follows:—It consists of a tube, the outer surface of which forms the inner surface of the drain. This tube is divided longitudinally into six or more parts or planks, the lateral divisions being of the same width throughout; the lower or bottom plank and the upper or vaulting piece only being wedge-shaped. The upper wedge must be, on the whole, narrower than the semi-circle of the vault, so as to enable the workman to detach it at the proper time from the concrete without pressure or loss of time. All the planks have smooth horizontal joints, and the tube formed of them is somewhat rounded off inwards, or drawn together at its front and back ends, so that its cross-sections at those places are somewhat smaller. This facilitates the insertion of the tube in front in a guage-ring of the drain-mould, and behind in the completed piece of the drain; at the same time it adapts the tube for making slightly-bent drains. The lateral planks are jointed to the gauge-ring by means of conic tenons in projections of the same; the bottom plank is secured tothe gauge-ring by two wedges. This gauge-ring cuts off the concrete to be brought in in such a manner that each new piece of drain is rabbeted to the piece last made.

The gauge-ring is adjusted by wedges, and at top and bottom by squares and plummets provided with exact marks. As the gauge-ring must always be at a right angle to the axis of the drain, it will, owing to the fact that sewers have more or less of a fall, and are, as a rule, constructed from below towards the top, be not vertical, but hang over at the top. In accordance with this, a mark corresponding to the inclination is placed upon the lower square, and the plummet set upon it. The upper square is put upon the correct longitudinal direction of the drain by means of sighting rods. The withdrawal of the apparatus after fixing the gauge ring is effected by first loosening the bottom plank and withdrawing it, and next securing it to the gauge-ring by means of the wedges mentioned, while, at the back, it is supported at the lateral planks still in the drain also by wedges. The lateral pieces are kept in their place by suitable wedge stays. As soon as the bottom plank is fixed the concrete is stamped in between the soil and bottom plank by means of curved pestles, and leveled with radial joints. The lateral planks are then drawn forward in a similar manner, fastened, and stamped in with cement. The vaulting piece is then similarly dealt with. The vaulting slab is fixed to a carriage-like wheeled frame, which follows on withdrawal. The vaulting piece settles somewhat, but is lifted again on being fixed to the gauge-ring. Two gauge-rings are only necessary at the commencement of work. The carriage is then put inside the tube, and connected with lateral pieces, for which it has supports. These longitudinal pieces serve for fastening the wedge stays, which secure the lateral planks.

Various sections, but mostly of an egg shape, have been made with this apparatus. The sewers of Linz are constructed of concrete of a thickness of 6.2 in. at the bottom, 5.9 in. at the sides, and 5.1 in. at the crown, and they have an inner height of 3.8 ft., and a greatest width in the upper quarter of 1.9 feet. The concrete used for them consisted of one part of Portland cement, one part of Kufstein cement lime, four parts of sand, and four parts of gravel. The municipality of

Vienna has all the sewers of the city constructed after this method. The concrete used for the bottom consists of one part of Portland cement, three parts of sand, and seven parts of broken stones; that for the lateral portions of one part of cement lime, two parts of sand, and two parts of broken stones.

STONE ARCHES UNDER EMBANKMENTS.

By B. S. RANDOLPH.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE cheapness and facility with which iron bridges are now built seems to have caused a very general decline in the use of the stone arch, notwithstanding the fact that an estimate of cost will frequently show a decided difference in favor of the latter, especially when the span is not very great, or when high abutments would be needed to support an iron superstructure. Added to this, the stone arch, when once properly built, needs no further care to correspond to the constant watching, painting and repairing required by iron structures.

Such modern examples of the stone arch as we have of less than fifty-feet span seem to be confined to the semicircular or "full center" form. This is very graceful and, while the crown is near the surface so that the load can be properly distributed, answers the purpose very well; but the frequent failures under high embankments indicate practically that there is room for improvement, a fact which will also become theoretically apparent when an effort is made to construct the line of pressure in a semicircular arch so loaded.

By line or curve of pressure is meant that line on which, if all the forces of the load be applied in their proper position, direction and amount, their resultants will maintain each other in equilibrium. Several methods of obtaining this are given by the authorities, as also the demonstration of the fact that it should lie at least one-third the depth of the ringstone from either end, or, as commonly expressed, "in the middle third," so they need not be repeated here.

The very interesting article of Mr. Benjamin Baker on "The Actual Lateral Pressure of Earthwork," together with the discussion which followed in the Institution of Civil Engineers, as published in VAN NOSTRAND'S MAGAZINE, October, November and December, 1881, show the futility of any calculations, in the present state of knowledge on the subject, of the character of pressure which is experienced by an arch under an embankment. Nor is more knowledge on the subject likely to decrease the difficulty of determining the proper shape of such an arch, since we know that certain materials give more lateral pressure when freshly deposited than after they have settled, while others behave differently when wet and when dry, without regard to the length of time which they have been deposited.

So the problem of drawing an arch of such form that the line of pressure shall lie in the "middle third" of ringstone of any reasonable depth becomes practically impossible, and the way out of the difficulty seems to be to control the load so that its character shall be constant and then proportion the arch to meet it.

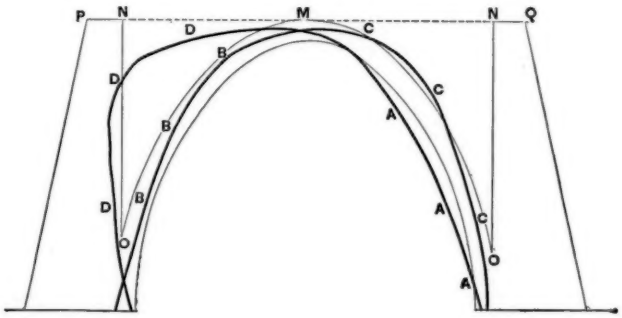
In some recent designs for arches under high embankments I have used the following method, which seems to accomplish the purpose, though I have not had time to test it practically.

The abutments are carried up to a level with or a little above the crown, as shown in the cut, having sufficient base to act as retaining walls, and resisting the lateral pressure, they allow nothing but vertical pressure to come on the arch.

In the construction the earth is brought up to the top of these walls, when the lateral pressure will cause them to move slightly inwards by virtue of the elasticity of the material of which they are built.

The space MNO, which has been left open until now, is filled with thin, hard, flat stone, loosely hand laid on their flat surfaces. In this way the lateral press-

ure is kept from the arch as far as possible, since the walls have moved as far as they are likely to, and the flat stone, while they will transmit any amount of vertical pressure, will move on themselves before transmitting very much lateral pressure. If this is deemed insufficient the space MNO could be built solid with stone laid in mortar, and an opening a few inches wide left on the line NO and covered with large stone laid over the top to keep the earth from filling it up. Supposing this arrangement of the load, the arch is drawn almost if not wholly for vertical pressures, depending on the engineer's confidence in his arrangement for securing such pressures.



In the cut are given the various forms of the pressure lines for extreme cases in an arch of the general form of the one shown.

For a very high embankment, supposing the pressure to be equally distributed, and all pressures vertical, we have the line A. Supposing the pressure equally distributed, but allowing pressure, in addition to the vertical, at one half the usual angle of repose ($56\frac{1}{2}°$ with the vertical) as in the calculations for retaining walls, we have the line B. Taking all vertical pressures but proportioned in amount to the amount of material below the line PQ, we have the line C. Allowing for pressures at the same angle as before, but proportioned to the amount of material below the line PQ, we have the line D.

It will be observed that these are extreme cases, so the true line for each case must lie somewhere between them.

If we could rely on the walls to do away with all the lateral pressure on the arch ring, the true line would lie between A and C, depending on the depth of the crown below the surface of the embankment.

This, it is scarcely safe to expect, in view of the elasticity of the materials composing the walls and of the tendency of the material between the walls and the ring to become somewhat compact under the vertical pressure and so transmit lateral pressure.

The form would then approach that in which lateral pressure was considered, which would seem to point to the line B, a medium between A and C as the one most likely to meet all conditions.

The following method of drawing the arch produces this form very nearly and will also be found to satisfy quite a number of various conditions of load. From the springing line as a center with the a radius equal to the span describe a segment upwards from the opposite springing line to a height of 45°. Draw the opposite side in the same manner and connect the two arches with one of 90° tangent to the first two, the radius of which will be .293 of the span.

This form of arch gives somewhat less area of opening than the full center form of the same span and height, but the diminution is principally in the upper part, which in large arches should not be considered in calculating waterway, and would make very little difference in the passage of most vehicles. On the other hand, in a full center arch, without an assurance of considerable lateral pressure or a sufficient difference in the amount of load at the crown and at the haunches, the construction of a few lines of pressure will show a very un-

stable condition of affairs, the line lying far inside the curve of the voussoirs tending to raise the haunches and let the crown down.

This fact is borne out by the failures of full center arches in actual practice, which usually occur by a dropping of the crown of the arch while at the haunches, being unable to rise against the load, the voussoirs are chipped and cracked on their inner surfaces by the excessive pressure near the inner surface of the ring and so make room for the descending crown.

From what has preceded it is not to be supposed that it is intended to state that a full center arch under a high embankment will always fail, since a variety of circumstances may, and do, obtain to make them stable.

In embankments composed largely of rock, gravel, or any latcose material there is always considerable lateral pressure, even when dry, which would cause the line of pressure to approach the shape of a full center arch. And beside this in the construction of most semicircular arches they are "loaded" over the haunches with stone laid in cement, which, on setting, converts the mass into more or less good masonry, so that the line of pressure may lie anywhere, either in the ring or "loading," and the structure be stable under a variety of conditions for which it was not strictly designed.

For instance, under a given load the shape of the curve of pressure depends on the ratio between the rise and span, and if we assume a segmental arch having a rise equal to one-fourth the span, we will find that it coincides very closely with the curve for a load of all equal vertical pressures. This curve might readily be contained in the ringstone and loading of almost any full center arch, and if we suppose a condition of load approximating this to occur in the embankment, the curve of pressure passing through the keystone will gradually diverge from the line of the ringstone and lying above them in the loading will reach the line of the abutment face at a point approximately one-half the rise above the springing line, and the arch will in reality act as a segmental arch with a rise equal to one-fourth the span, the ringstone near the springing line carrying nothing but their own weight. This, of course, gives a very considerable lateral pressure which the abutments with such assistance as they obtain from the material placed behind them may be able to resist, in which case the structure will show no signs of failure, more through accident than intention. Such a structure, while it might carry its load for an indefinite length of time, would scarcely be creditable to a professional engineer, whose aim should be not only to accomplish his object thoroughly and effectively, but to do it with due regard to the amount of money expended, and frequently to practice the strictest economy, neither of which could be said to have been considered or practiced in a structure in which some of the parts would never be called on for anything but the support of their own weight.

A Monument to Alexander Lyman Holley.—The worthy project of erecting a monument in Central Park to the memory of Mr. Holley is announced by a circular issued by direction of a joint committee, composed of special committees from the American Society of Civil Engineers, American Institute of Mining Engineers, and the American Institute of Mechanical Engineers.

It is proposed that the monument consist of a suitable pedestal in stone, surmounted by a portrait bust in bronze. The cost will be about ten thousand dollars.

The sub-committee, to whom is entrusted the power of receiving subscriptions, is composed of Chas. Macdonald, R. W. Raymond, and J. C. Bayles. The office of the treasurer, Mr. Macdonald, is 52 Wall Street.

The Rensselaer Polytechnic Institute.—The plan of raising an endowment fund for this institution is meeting with encouraging success. The amount of thirty-one thousand dollars is already pledged.

The committee regard with much satisfaction the fact that a warm interest in the project is manifested by the Alumni of the Institute, and that a larger portion of the fund thus far pledged is made up of moderate sums subscribed by graduates who are actively engaged in engineering.

THE RESISTANCE OF VIADUCTS TO SUDDEN GUSTS OF WIND.

By JULES GAUDARD, Civil Engineer, Professor at the Academy of Lausanne.

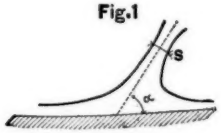
Translated from the French by L. F. VERNON-HARCOURT, M.A., M. Inst. C.E.

In order to ascertain the condition of stability of a structure exposed to wind, it is necessary, in the first place, to know the pressures which atmospheric disturbances can produce, and then to study the effects of these forces, and the additional strength necessary to resist them.

With regard to the first part of this programme, it is essentially necessary to have recourse to experience. In fact, its only theoretical basis is a doubtful similarity between a gaseous jet and a stream of liquid, which latter, though a more simple phenomenon, admits only of approximate investigations.

When a fluid stream, whose cross-section is s and velocity v, strikes against a plane surface, to which its axis is inclined at an angle a, it spreads out in a layer against the obstacle, as shown in Fig. 1;

Fig.1



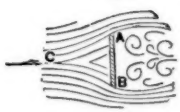
and the formula which expresses the total normal pressure on the surface is $\frac{\Pi v^2}{g}$ $s \sin a$, in which Π denotes the specific weight of the liquid, and g the acceleration due to gravity. As $\frac{v^2}{g}$ is double the height which the column of water would require to fall to attain a velocity v at the bottom of the fall, it follows that the dynamical pressure, in the case of vertical incidence, may amount to double the weight of the same column in a state of rest. The pressure, moreover, is reduced in striking against a convex surface, and increased against a concave surface.

This phenomenon was said to be comparatively simple, because the liquid, owing to its high density, is little affected by the surrounding medium of air which it displaces, or against which it rubs. Moreover, for a stream of small section, the surface is assumed to be much larger than the section, in order that the spreading out may be complete.

If now a plate having an area S, is struck by the air, the gaseous stream will have a cross-section, S sin a, limited merely by the circumference of the plate; the central filaments will always find an ample surface over which to spread, but in doing this they will push out and turn aside the other filaments; and as regards the outside filaments, their position will be so far different that, with only a very slight deflection, they will escape before having exerted all their dynamical force. On the other hand, the column of air arrested by the obstacle will be hemmed in by other layers of air in motion, which it will whirl about in forcing a sideways outlet for itself. Lastly, the partial vacuum produced on the sheltered face will enter as a cumulative force into the problem of stability. If these disturbing conditions could be neglected, taking as an average $\Pi = 1/k$ 225, and $g = 9.81$, the formula of the fluid stream would give, per square meter of surface impinged on, a pressure of $0.125\ v^2 \sin^2 a$, produced by a wind having a velocity of v meters per second, and with an angle of incidence a.

In reality the numerical factor may differ more or less from this theoretical result; but, as regards the degree of influence of the velocity and the mass of the fluid, it appears to be confirmed by the following considerations. An obsta-

Fig.2



cle AB (Fig. 2), being placed in the course of a fluid, the filaments CA, CB, diverge in curved lines, turning their

convex side towards the obstacle. This curvature produces centrifugal reactions proportional to the mass of the molecules and to the square of their velocity; and it is the sum of these reactions which develop the "live pressure" against the front face of the body AB. At the opposite side of AB, on the contrary, the filaments AD, in tending to return to the line of their former direction, assume curves with the concave side turned towards the obstacle. Accordingly a partial vacuum, or "non-pressure," as Dubuat terms it, is produced, which has an effect similar to the "live pressure," and is additional to it in the final result.

The specific weight being $\frac{\Pi}{g}$, the total resistance may be expressed by $K \Pi \frac{v^2}{2g}$, or 0.0625 Kv^2 per unit of surface, in which the value of the coefficient K must be determined by experiment. If the plate AB is replaced by a prism more or less elongated, the "live pressure" remains the same, but the "non-pressure" is reduced, and consequently the value also of K, which represents the resultant of both forces. Thus Dubuat, who had obtained K=1.43 for a plate moving in a liquid, obtained similarly K=1.17 for a cube, and K=1.10 for a prism whose length was thrice one of the sides of its base.

Experiments made in air appear to have given results varying between K =1.3 and K=2.2 in the case of thin plates; variations due perhaps, partly, either to the inexactness of the law of the square of the velocity, or to the influence of the size of surfaces employed, or to the rotatory motion of these surfaces in the experiments when they are paddles of wheels.

General Morin has introduced a constant into the formulæ. From experiments made at Brest, in 1823, by Thibault, by means of a fly-wheel with little sails on a horizontal axis, he deduced the formula $0.0044 + 0.108v^3$ as expressing the resistance per square meter. The coefficient 0.108 remains practically constant for inclinations between 90° and 50°, provided it is referred to a square meter of surface projected on a plane perpendicular to the direction of motion.

In 1835–37 Piobert, Morin, and Didion, made observations on the fall of a plate suspended to a cord; the laws of the motion being indicated, with respect to the guide pulley, by a clock-work apparatus. The resulting formula, namely, $0.036 + 0.084v^2 + 0.164j$, contains a term proportionate to the acceleration j in the case of variable motion, which vanishes for uniform motion. Analogous formulæ have been obtained for parachutes. The velocities observed did not exceed 10 meters (33 feet) per second.

Whereas for slow motion the law of pressure appears to be best expressed by formulæ having two terms, of which one is proportional to the square of the velocity, and the other is taken as a constant by some, or proportional to the simple velocity by others; it is found, on the contrary, that the intensified phenomena of ballistics indicate a greater variation than the square of the velocity. Piobert estimates the resistance to motion of a projectile whose section is s as $0.023\ sv^2\ (1 + 0.0023v)$; but sometimes it is expressed by a single term proportionate to v^3. As regards the reduction of pressure due to the obliquity of the current, experiments indicate a less rapidly diminishing factor than the square of the sine. Didion found that in bending the opposing surface so as to form a convex two-sided angle, and inclining each of the two faces thus formed at the same angle a to the direction of motion, the formula has simply to be multiplied by $\frac{a}{90°}$, so long as a is between 90° and 65°.

Hutton had arrived at the complicated formula $0.135\ s^{1.1}\ v^2\ (\sin a)^{1.84 \cos a}$ for the total pressure upon the surface s of a plate in the case of velocities below 10 meters (33 feet). It will be noticed in this formula that the pressure per unit of surface is considered to be proportional to $\frac{s^{1.1}}{s}$ or to $\sqrt[10]{s}$, which agrees with Borda's experiments, which indicated a pressure of $0.09v^2$ per unit of surface, on a square whose side was 0.11 meter ($4\frac{3}{8}$ inches), and $0.105\ v^2$ when the side amounted to 0.25 meter ($9\frac{7}{8}$ inches). The influence of the size of the area on the result is explained by the fact that

the filaments of the current near the sides only produce a partial effect, and the larger the surface, the smaller is the proportion of the perimeter to the area. However, Didion, Thibault, and other observers, have, on the contrary, arrived at the conclusion that the total pressure is proportional to the surface, and independent of its form. Morin gave as an objection to Borda's experiments, made with a fly-wheel having small sails turning a vertical axis, that the effect of the friction of the apparatus had not been calculated.

The resistances offered by the air to railway carriages in motion have been variously estimated: Thus Harding gives $0.0627\ v^2$, and Ruehlmann $0.117\ v^2$ per square meter of front section. The circumstances, however, are complex, and when it is desired to estimate the resistances as closely as possible, it is necessary to go into the details of the carriages in order to ascertain the effect of the air in the spaces between them.

It is generally accepted as an axiom that the resistance offered by air at rest to a moving body is equal to the pressure which wind moving with the same velocity would exert on the body at rest. Smeaton, adopting a table drawn up by Rouse for winds having velocities not exceeding 72 feet per second, appears to have accepted pressures denoted by the formula $0.0023\ v^2$, which are given in a tabular form in the Minutes of Proceedings, vol. v., p. 292. In the same volume (p. 296) will be found the results of the careful experiments made by Colonel Beaufoy in 1815, with plates however only 1 foot square, which may account for these pressures being less than those adopted by Smeaton. General Morin deduced a formula from some experiments by Thibault in 1826, which gives results approximate to those of Smeaton, but decidedly greater than the resistances experienced in moving flat discs in still air, which would support Dubuat's opinion as to the incorrectness of the axiom mentioned above.

It would appear from calculation that the pressure on a cylinder is two-thirds, and that on a sphere half of the pressure on their diametral sections. Borda, however, obtained by experiment the smaller values 0.57 and 0.41 as the relations of these pressures. For a prism presenting a right-angled isosceles triangle to the air, he obtained the proportion 0.73, and for a cone the values 0.69 or 0.54, according as the angle at the apex was 90° or 60°.

The velocity of the wind is recorded by anemometers. Thibault obtained the pressures by plates attached to springs for measuring the resistance.* In a similar manner Mr. Paris took measurements of the wind at sea by fastening small boards to a deal rod which served as the spring, and he obtained the following results:

FEET PER SECOND.

Velocity of the wind	2.6	5.9	11.2	19.7	30.2	43.0	59.7	77.4	98.8	125.3	150.9

LBS. PER SQUARE FOOT.

Pressure exerted.....	0.02	0.09	0.30	0.87	2.13	4.25	8.03	12.56	22.12	37.90	51.20

These figures approximate to those given by Smeaton's formula, and are smaller than those derived from Hutton's formula, which formulæ would give for a great storm of 151 feet velocity per second about 51.8 lbs. and 57.3 lbs. per square foot respectively. In the higher regions of the atmosphere the velocities may be very great, as it is stated that, in 1823, Green traveled in a balloon at the rate of 210 feet per second.

The absolute relation between the pressure and the velocity is by no means

* Dr. Lind, in 1775, employed a reversed siphon containing water; and the wind entering one branch made the water rise in the other branch, thus affording a measure of the pressure exerted. (*Vide* Minutes of Proceedings Inst C.E., vol. v., p. 290, and Philosophical Transactions, 1775, p. 353.—L, F. V.-H).

indispensable for ascertaining the stability of structures exposed to the wind. It is sufficient for this purpose to find the greatest pressure that may occur in a given locality during a sudden squall.

Rankine states about 55 lbs. on the square foot as the greatest wind-pressure observed in England by anemometers or dynamometers, which is confirmed by the fall of chimneys and other buildings. However, a pressure of 61 lbs. on the square foot was recorded at Liverpool during the storm of the 7th of February, 1868, and of 71 lbs. on the 27th of September, 1875.

The violent storm of 1876, which overturned several chimneys in Germany, was reckoned to have a velocity of 102 feet, and a direct pressure of 29.5 lbs.; but, taking into account the "non-pressure," due to suction at the back face, it is estimated that the total resultant pressure on these structures must have been a third more, and consequently equal to 39.3 lbs. per square foot.

The upsetting of a train between Narbonne and Perpignan, in December, 1867, indicated a pressure of between 30 lbs. and 50 lbs.; and other similar accidents with empty wagons on the same railway in February, 1860, and January, 1863, indicated a pressure of from 25 lbs. to 33 lbs. No other part of France is exposed to such violent storms; nevertheless, in considering the stability of light-houses, Fresnel allowed for the possibility of wind-pressures up to 56 lbs.

It would appear that American engineers, for the resistance of bridges, assume wind-pressures of 30 lbs. per square foot upon the loaded and 50 lbs. upon the unloaded structure, although certain local tornadoes in that country might have exerted forces amounting to as much as 84 and even 93 lbs.*

Instead of waiting for chance accidents, which have to be investigated after the event with inadequate data, it would be advisable to set up apparatus at once in certain meteorological observatories for registering the pressure of great gales. For example, a kind of case of pigeon-holes might be placed in windows facing in a suitable direction, these holes being closed by a series of little shutters one above the other, capable of moving inwards under certain pressures of wind, being guided by little rollers, and made to close again against the external rabbets of their respective frames by springs or counterpoises with suitably gradauted power. Lastly, each of these movable panels might be so arranged that the moment it began to open it should unhook a signal which would bear evidence to the movement even after it had closed again. It would suffice after each storm to ascertain, by a rapid inspection, which of the panels had yielded to the wind, and then whichever of these panels offered the greatest resistance would measure the pressure experienced.

Of all engineering structures, suspension bridges are the most easily acted upon by wind. Their primitive methods of construction were defective through excessive flexibility. The accident which happened to the Roche-Bernard bridge on the Vilaine, on the 26th of October, 1852, and the successive injuries to the Menai bridge in 1826, 1836, and 1839, may be cited as examples. The chains of the latter bridge, though clashing together violently, bore the strain; but a number of transverse pieces and suspension rods broke, and 160 feet of flooring hung in the air in 1839. According to the bridge-keeper, the undulations of the roadway attained an amplitude of 13 or 16 feet, and the greatest deflections were observed at the distance of a quarter of the span from the piers. It is evident that everything gives way in these irregular undulations, which are different for the chains and the roadway. The Menai bridge was strengthened by various means. The Roche-Bernard bridge was provided with a counter cable, curving upwards and placed under the roadway; and notable progress has been achieved in the design of more recent works. The Americans, in developing the principle of the stiffening girder, have also added a series of straight and sloping cables coming from the top of the piers and supporting various parts of the roadway. They have, moreover, in some large bridges, anchored the roadway to the rocks by stays underneath, a method which is not free from objections any more than the parabolic counter cable

* Minutes of Proceedings Inst. C.E., vol. lxiv., p. 352, and vol. lxvi., p. 388.

of the Roche-Bernard bridge, for the variations in temperature may at one time loosen and at another time stretch these understays.

In the Ordish system, as applied to the Albert bridge, Chelsea, the upper stays, starting from the tops of the piers and ending at various parts of the roadway, are connected with the vertical suspension rods at divers points of crossing, which increases the total rigidity. Sometimes, as at the Lambeth bridge, rigidity is obtained by the introduction of cross bracing or diagonal bars between the suspension rods; or, as at Pittsburg, the chain itself is made rigid, assuming the appearance of two sloping lattice girders of variable height, and attached to their narrow extremities, at one end to each other in the center of the span, and at the other end to the tops of the piers.

The great transversal inclination in certain bridges to the two funicular planes, by which the cables, spreading out at the tops of the piers, come together in the center of the span, affords a powerful resistance to lateral oscillations.

With these improvements the suspension system, without losing its inherent lightness, is protected from irregular undulations when exposed to wind; so that the wind pressure merely acts on it, like on any other structure, in producing an increased molecular strain which has to be provided for by strengthening the parts liable to be affected.

It is true that a great number of suspension bridges exist which were constructed on the old flexible principle, and have stood for many years; but their preservation is doubtless due, in most cases, to their not having experienced the full force of the wind whirling under their roadways, owing to their small height above the water, or other circumstances. The most exposed bridges are those which traverse deep and shut-in gorges at a great height.

Wind has no effect on massive stone bridges; but every light bridge, whether of iron or wood, although rendered rigid, is liable to side strains, or small elastic vibrations producing molecular deformations, upon which the conditions of resistance of the material depend.

Though the motion of wind is generally parallel to the ground, its action on the underside of the roadway may become considerable, owing to the rebound of the wind from the bottom of ravines, which occasions the great danger to light flexible suspension bridges of being raised and falling again violently. When the wind, blowing in sudden gusts, lifts the platform slightly, the platform falls again for a moment below its normal level to a similar extent, so that the pressure of the wind from below produces eventually the same strain as if its action was added to the load. Accordingly, in special cases, where it might be possible to estimate at an appreciable amount the vertical resultant of a storm beating against the roadway of a bridge, it would be correct to treat it as an extra load on the bridge.

The effect might be still more serious in a bridge with several continuous spans, for, as nothing could ensure the concordance of the oscillations of the various spans, it would be necessary to provide against the worst case of a pressure from above on certain spans aggravated by a pressure from below on certain other spans.

Putting aside, however, these accessory or derived effects, let the wind be considered solely in its horizontal direction, in which it displays its greatest power, and, knowing its force on a single solid surface, let an endeavor be made to calculate the force exerted on several open, or partly open, surfaces.

Taking the case of a bridge consisting of two solid girders, though these girders cover each other completely in a geometrical sense, yet the first, whilst exposed to the full force of the wind, does not completely shelter the other. Thibault experimented on two square screens covering each other, and placed at a distance apart equal to the length of one of their sides, and found that the wind pressure on the one screen being 1, a total wind pressure was experienced on the two of 1.7 In the case of a bridge, the wind pressure cannot be so high, as instead of four edges there are only two at the most (when the platform is halfway up the girders), round which the wind can whirl and beat against the second surface; the coefficient of in-

crease in such a case, deducted from the preceding instance, will perhaps amount at most to 1.4. It would be reduced to 1, and even less, if the girders were connected by solid platforms at the upper and lower edges. Lastly, in the case of a single platform, placed at the top or the bottom, it would be perhaps necessary to estimate the total lateral pressure as equal to 1.2 time that which the side directly exposed would experience. It is evident that if a train is on the bridge at the time when the storm is raging, the resistance that it offers to the wind aggravates the strains on the structure.

Considering, now, the case of trellis girders, each opening may be regarded as an orifice, with thin sides, through which a jet of air rushes; there will be some contraction of the fluid vein, and the side will experience a little greater resistance than the ratio between solid and void would indicate. If p denotes the wind pressure, s the whole surface of the side of the girder, σ the open portion of this surface, and k the coefficient of contraction, the pressure on the girder will be $p\,(s-k\sigma)$. The value of k, according to D'Aubuisson, would equal 0.65 for small orifices, but as it doubtless varies inversely as the ratio of the perimeter to the surface, which diminishes as the dimensions increase, it may be assumed that k approaches unity in the case of large openings. However, as its real value is not known, it will be better to risk exaggerating it in the case under consideration.

Suppose, now, that a second side exactly similar is placed behind the first, it receives the shock of the portion of wind which has passed through. This wind may be considered to have been made homogeneous by the whirling which occurs in the interval between the two girders, and to have a reduced force $p\frac{k\sigma}{s}$, according to the relation between the amount of air which has traversed the first girder and the total original mass. Consequently the second trellis will experience a pressure $\frac{pk\sigma}{s}(s-k\sigma)$; and similarly the wind which passes through it will have its force reduced to $p\left(\frac{k\sigma}{s}\right)^2$. If there are n successive girders, the sum of the pressures experienced will be

$$p\left(s-k\sigma\right)\left(1+\frac{k\sigma}{s}+\frac{k^2\sigma^2}{s}+\ldots+\frac{k^{n-1}\ \sigma^{n-1}}{s^{n-1}}\right)$$
$$=p\frac{s^n-k^n\,\sigma^n}{s^{n-1}}.$$

As the above calculation does not take into account the wind which may come round the sides of the front girder, a certain coefficient must be introduced, smaller than in the case of solid girders, as some opposition is offered to the inflowing wind by the wind passing through the girder. Perhaps the coefficient 1.10 would amply suffice in the majority of cases.

Another process of approximate calculation of the pressure of wind on a trellis girder has been employed by Mr. Nordling. He assumes that the filaments of air slant a little, so that those which pass through the openings of the first girder strike against the solid portions of the second. In this way a succession of trellises would finally act as a solid girder, when no openings are visible in a direction only slightly deviating from the normal.

Having ascertained the lateral force exerted by the wind against the roadway of a bridge, it is necessary to calculate the special molecular strain which it tends to set up, in order to add it to that produced by the permanent and moving loads. In resisting the wind, the roadway acts as an imaginary girder whose flanges are the actual girders of the bridge, and whose lattices are the horizontal braces and wind ties. The resistance, moreover, offered by the irregular interlacing motion of the trains must be taken into consideration. Owing also to the wind coming in gusts, thus causing a reaction, its effect on each girder, whether tensive or compressive, must be considered as added to the strain due to the load, and in the case of several spans the most unfavorable condition must be allowed for.

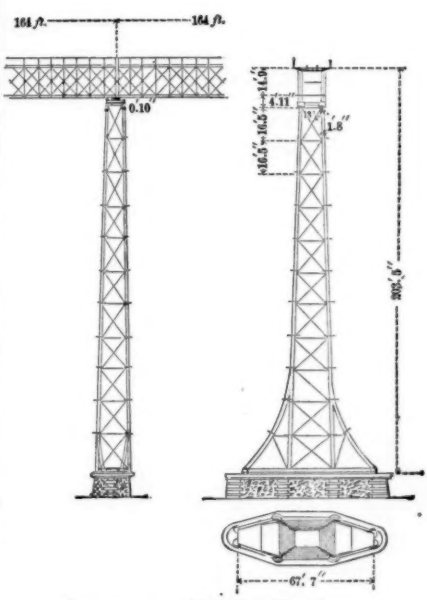
An arch has the advantage over a straight girder of opposing less surface

to the wind in the central portion, whilst the opposite is the case with a bow-string.

Two examples of iron arches, with narrow roadways, spanning very large openings, are those of Oporto, on the Douro, which has a width of 14 feet 9 inches between the parapets and a span of 525 feet, and that of the Montereale, on the Cellina torrent, which has a width of 9 feet 10 inches and a span of 272 feet. But these bridges are secured against the wind by special contrivances; the first, by giving a batter of 0.1164 to each face of the bridge, so that the distance from center to center of the arched ribs, which is only 12 feet 10 inches at the crown, is increased to 49 feet 2½ inches at the springings; the second by an external wind bracing, namely, by side buttresses coming from the haunches of the arch, and butting against the masonry at two points 27 feet 7 inches apart, whereas the distance between the arched ribs is only 9 feet 10 inches.

Certain structures may be liable to be wholly overturned by a gust of wind. Iron superstructures are generally free from this danger in consequence of their weight, except perhaps during a dangerous stage in some methods of putting them in place, especially if detached girders are being moved. On the contrary, the iron piers of very high viaducts need to be very firmly anchored in their masonry pedestals, as Mr. Nordling has pointed out in his memoir about various works on the branch lines of the Orleans Company. These kinds of piers are eventually strained as elastic braced structures fastened at their base and subjected at their summit to violent horizontal thrusts. On this account, instead of distributing their mass in a number of external and internal uprights, it is better to concentrate it at the angles in only four ribs connected together by cross-bracings. The anchorage at the base is rendered more economical, or more powerful, by fastening buttresses to the piers near their foot so as to enlarge their base. If the height does not exceed 130 feet, as for instance at the Bellon viaduct, the uprights may be curved outwards towards their base, so as to spread out without the aid of special stays. It would be equally feasible to secure the tops of high piers by stays fastened near the top of the piers and firmly anchored to the ground; but the system of buttresses is more æsthetic, and is not liable to get loose. One of the high piers of the Bouble viaduct, Fig. 3, will serve as an example to illustrate, by an approximate process, to what severe strains such a structure might occasionally be exposed. Mr. Nordling has assumed the wind pressure at 55.3 lbs. per square foot, without allowing for a train on the bridge, as, in his opinion, if such a

Fig.3



PIER OF THE BOUBLE VIADUCT.

storm ever burst upon these structusre the traffic would be suspended for a time; and, moreover, the above pressure appears to him excessive for the locality. Let, however, the worst possible case be considered by imagining a concurrence of adverse circumstances, the structure being in a very exposed situation, and the full fury of the gale suddenly occurring whilst a train is passing over.

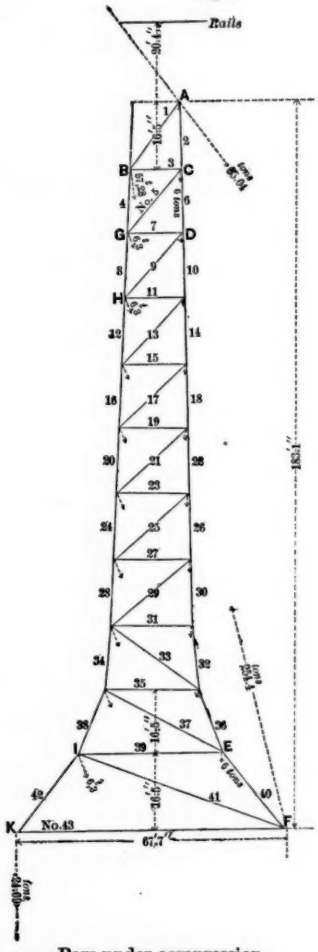
Taking only a half pier containing two uprights and the intermediate bracing, the span being 164 feet, crossed by two lattice girders 14 feet 9 inches high,

it appears that, allowing for the spaces, the wind, having a pressure of 55.3 lbs., would exert a total stress of about 20 tons at a height of 196.2 feet above the footings, which gives a moment of 3,924. The pressure on the train is 16.2 tons, with a leverage of 210.3 feet, giving a moment of 3,407. Lastly, the moment of the pressure of the wind on the half pier amounts to 20 tons $\times$ 92.85 feet = 1,857. Thus the total moment of overturning on the edge of the base is 9,188. The moment of stability due to the loads is obtained as follows: taking 60 tons as the weight of the half span, and 120 tons as the weight of the half pier (the cast-iron cylinders being ballasted with concrete), and allowing 42.5 tons as the weight of the train which suffices to prevent its being overturned by the gale, the total weight amounts to 222½ tons, and the half width of the base being 33.8 feet, the moment is 7,520, leaving a deficiency of 1,668. To provide for this the anchorage tie must exert a tension of $\frac{1,668}{67.6} = 24.69$ tons. Without the help of the buttresses the width of the base of the pier would be only 24 feet 3 inches, instead of 67 feet 7 inches, and the anchorage would be subjected to the great strain of 267 tons.

In order to form a notion, not merely of the strain on the anchorage, but of the strain on the whole structure of the half pier, a graphic illustration is given of the polygon of forces, considering, for the sake of simplicity, the imaginary case of an articulated structure. The lattice, moreover, is hypothetically reduced to the lines of Fig. 4, by omitting as well the foot of the straight uprights, replaced by the corresponding curved or polygonal stay, as in each row of bracing, that of the two diagonals which, exposed to a wind from the left, would be strained in compression, and are considered to be too flexible to offer an effectual resistance in this way.

The external forces applied to the various summits produce the following horizontal components. At the summit A the whole force of the wind against the beams and the train is brought to bear, namely, a force of 40.04 tons obtained by dividing the moment, 7,331, by the height, 183 feet, of the point A above the base. The pressure against the half pier amounts to about 2 tons acting at each of the points B, G, H, . . . I, situated on the side which the wind strikes. The weights or vertical components are:—

Fig. 4



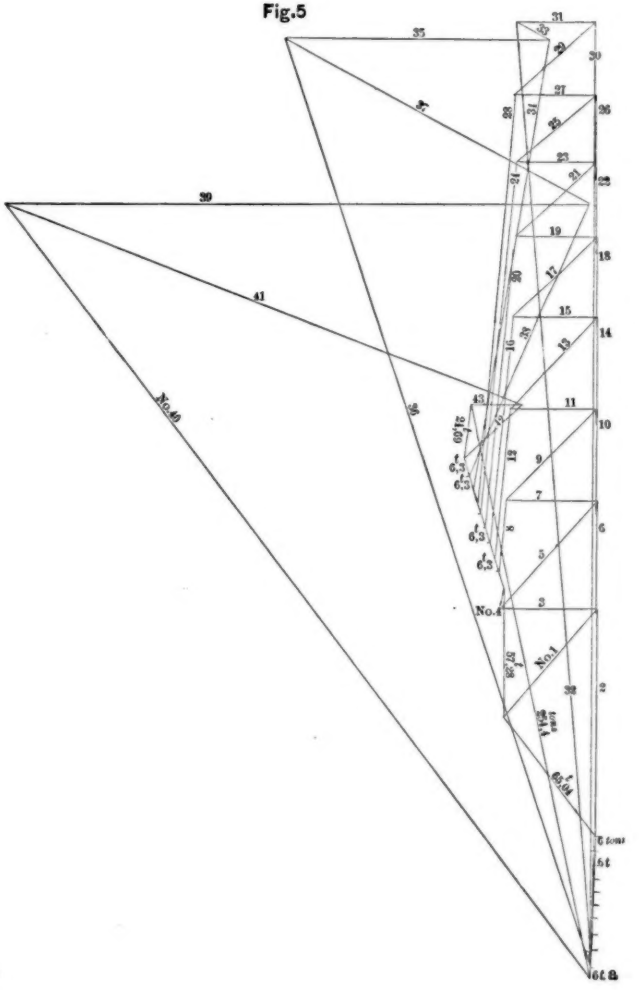
THEORETICAL STRUCTURE OF THE PIER.

51.25 tons at A, due to the loaded roadway; the same weight at B increased by a portion of the pier, amounting altogether to 57.25 tons; lastly, in each of

the points G, H, . . . I and C, D, . . . E, a vertical force of 6 tons. The reactions in equilibrium developed by the base of support are: at K, the tension of anchorage, amounting to 24.69 tons as calculated above, acting from the top to the bottom: in F, a vertical upward reaction equal to the total weight increased by the strain of anchorage, namely, to 247.2 tons; and a horizontal force acting from right to left, which, counteracting in projection all the wind pressures, is equal to 60.04 tons.

The resultants at the different points consequently assume oblique or vertical directions. The oblique resultants are:

Fig. 5



65.04 tons at A; 6.3 tons at each of the points G, H. . . . I of the left upright; and 254.4 tons at the point F of the right upright. The state of equilibrium of the external forces is shown by a closed polygon in Fig. 5. Moreover,

this figure is completed by the addition or grouping of a series of other closed polygons representing the respective states of equilibrium of the various summits of the articulated system of Fig. 4, under the influence of the internal and external forces acting on each of them. The inscription of identical numbers in Figs. 4 and 5, serves to indicate their connection; thus, for example, the closed polygon 8, 9, 11, 12, 6.3 tons in Fig. 5 proves that the point H of Fig. 4 is in equilibrium under the external force 6.3 tons, the tensional strains of the bars Nos. 8, 9, 12, and the compression of the bar No. 11, the intensities of the forces being measured by the size of the lines on the diagram, Fig. 5. It will be observed that the left side is in tension from G to K, the greatest tensional strain, of about 190 tons, occurring on the portion No. 34. With a cast-iron pipe having an external diameter of 1 foot 8 inches, and an internal diameter of 1 foot 4 inches, this strain would amount to 1.9 ton per square inch; but, as previously stated, the Bouble viaduct was constructed on the supposition of the maximum pressure being less. The compressive strain reaches 422 tons at the portion No. 40, which would amount to 4.1 tons per square inch, but in reality the strain is less if the uprights are made complete, as shown in Fig. 3.

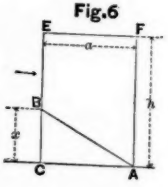
Moreover, it is certain that the rigidity of the cast-iron columns and their bolted flange-joints must considerably modify the conditions of the problem. Instead, therefore, of merely comparing the pier to an articulated system, each member of which is considered to be free to deflect in any way, as assumed above, it would be necessary, in a complete design, to study the transmission of force resulting from impeded deflections.

In certain mechanical structures, as, for instance, in swing bridges with short tail ends, the action of high winds may stop or impede their motion without actually producing any dangerous amount of damage.

High timber stagings, owing to their lightness and the broad surface presented by their planks, are exposed to considerable risks of damage by wind. An excellent method for strengthening them was adopted at the Chaumont viaduct, which is 164 feet high, and has three tiers of arches, each of which was provided with a temporary platform for the supply of materials. The staging was braced in various directions by iron wire cables, very tightly stretched and firmly anchored.

When a structure rests without sufficient adherence on a fixed base, a lateral thrust would turn it over by detaching it from its support; but if its fall cannot be effected without some indeterminate or chance cleavage, the rupture will take place in an oblique and downward direction B A, Fig. 6, because

Fig. 6



a certain triangular prism, BAC, possesses a stable position, on account of the leverage of the weight being great, and that of the impact of the wind small, in relation to the axis of rotation.

In reality, so long as the solid is not broken, the pivoting does not tend to take place on the extreme edge A, but upon some neutral axis of the section of rupture AB; as in every prismatic body, subjected to a bending strain, fracture results from the crushing of some portions and the tearing of others.

The direction AB being defined by the indeterminate $CB=x$, the external forces acting are, the weight of the prism ABEF, and the pressure of the wind on BE. In calculating the combined effects of pressure and flexure exerted on AB, the chance of fracture would be investigated from the position of the critical point A or B. The first of these points is the place of maximum compression; assuming that it reaches the limit of imminent crushing, an equation of ultimate resistance could be formed containing x and the pressure p of the wind per unit of surface as the variables. Then, by finding what value of x in this equation would make p a minimum, the direction of rupture would be obtained, provided that it is the point A where the disintegration begins. Such would be the con-

condition of a building very much strained by its own weight before the intervention of the wind.

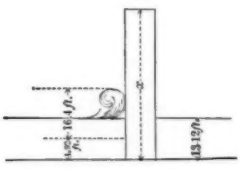
Under other circumstances, however, the point B might eventually be subject to a tension liable to prove more dangerous, though smaller in amount than the pressure at A, owing to the material being less able to bear tension than compression. It would be necessary, therefore, to examine the equation of rupture with regard to the point B, which might lead to another value of x applicable to the case where the disintegration commenced at this edge.

Nevertheless, nothing indicates that the fracture must be a plane surface. It might possibly slope somewhat in a homogeneous body; and in a masonry structure the fracture would run along the joists in some zigzag line; and these considerations limit the value of theoretical investigations.

Another reason for avoiding putting down the equations is, that they would lead to the disputed question of ultimate resistance in the complicated case of a material opposing an unequal resistance to tension and compression. With reference to the practical and legitimate need of a method or formula of safety applicable to the case in question, it is allowable to start on the simplifying hypothesis, commonly admitted in investigations of the stability of masonry, of the absence of cohesion, or neglect of the resistance to tension. If the line AB, whatever its direction, is regarded as a pre-existing fissure, the initial effect of the gust of wind, instead of being a pivoting on some neutral axis, would be from the first a rotation on the point A itself, at least, if the slight crushing of the edge is neglected. If, for example, Fig. 6 represents a wall with a rectangular base, the equation of actual equilibrium of rotation is $p\frac{h^2-x^2}{2}=\Pi a^2\frac{3h-2x}{6}$, where Π is the weight of a cubic foot of masonry. To remain stable against a given wind pressure p, the wall must have a thickness a sufficient for the most dangerous value of x. Now the value of x, which makes a a maximum in the above equation, is given by $x^2-3hx+h^2=0$, or $x=0.382h$; and the corresponding thickness is $a=1.0705\sqrt{\frac{ph}{\Pi}}$. If, for instance, $p=55.3$ lbs. per square foot, and $\Pi=150$ lbs. per cubic foot, the proper thickness would be $a=0.65\sqrt{h}$, where a and h are in feet. With this value there would remain the cohesion, which has been neglected as a factor of safety; and there would be no fear of the occurrence of extensions or of fissures, since, even with pre-existing fissures, the wall would not stir. If, however, a greater degree of stability was requisite, it would suffice to increase a by an optional amount.

An interesting instance of oblique rupture, caused, not by the wind, but by a stroke of the sea, occurred on the 8th of January, 1867, to the masonry tower beacon of "Petit Charpentier" at the mouth of the Loire. From an investigation of this accident, Mr. Leferme arrived at the conclusion that the pressure exerted by the blow of the wave must have amounted to about 6,140 lbs. per square foot. Mr. Thomas Stevenson, M. Inst. C.E., deduced some equally high pressures from observations at the Skerryvore rocks, which appear to be confirmed by the jets of water sometimes dashed to a height of 100 feet against lighthouse towers. Nevertheless, in most storms, and in most sea-coasts, the dynamical pressures exerted by the shock of the waves are generally estimated not to exceed from 600 1,000 lbs. per square foot. Even with this reduced value it is questionable whether, in the case of lighthouses and other structures in the sea, the wind pressure is not less dangerous than the shock of the waves. Taking the latter at 1,024 lbs., and the wind pressure at 55.3 lbs. per square foot, and assuming a tower to be immersed 13 feet (4 meters) in the water (Fig. 7), to what height would the tower have to be raised

Fig.7



for it to be in as much danger of being overturned by the wind as by the waves? The sea in a storm would perhaps rise 8.2 feet (2.5 meters) above its ordinary level; and if the smaller pressure on the bottom 5 feet (1.5 meter) is neglected, the total pressure on a height of 16.4 feet (5 meters) would amount to 16,794 lbs. With a leverage of 13.12 feet (4 meters) the over-turning moment with respect to the base is 220,000. Now, supposing the height of the tower to be x, the portion out of water will be exposed to a wind pressure of 55.3 lbs. $(x-13)$, and the moment of this force, $27.65\ (x^2-169)$, will only become equal to the former moment when x reaches the height of 90¼ feet.

If the same calculation is repeated on the assumption that the shock of the sea has its greatest possible degree of intensity, namely, that the wave rises 13.12 feet above its ordinary level, and exerts a pressure at the same instant of 6,144 lbs. on the whole height of 26¼ feet, the corresponding moment of 2,116,000 could not be equaled by the wind pressure on a tower less than 277 feet.

On the contrary, in the case of a viaduct only opposing a resistance to the water at the lower extremities of its piers, whilst the wind beats upon the lofty superstructure as well as against the piers, there is in all probability more danger to be apprehended from the wind. As to the conditions under which the Tay bridge catastrophe occurred, the author is not in a position to discuss them.

THE WATER-METER SYSTEM AND WATER METERS.

By MR. JOHN COLEMAN.

Abstract of a Paper read before the Society of Arts.

STEAM engines now pump millions of gallons of water through vast pipes, often spanning wide rivers, or rising over hills and sinking into vales, enabling water to be conducted under immense pressure. Gigantic reservoirs now exist, containing many days' supply, and aqueducts of stupendous proportions cross rivers at a cost of millions. In the streets of cities millions more have been expended for the great distributing pipes, until, to supply water for the necessities of life, the cost amounts to sums which seems almost fabulous.

Notwithstanding all this expenditure, gallons are run off to obtain a single glass of water, pipes are left open in sinks and closets, while few reflect that every gallon brought into a city and forced to high buildings is sent there at the expense of the taxpayer.

They do not comprehend that if five gallons of water are wasted for the one gallon really needed by all consumers the public works and the water taxes must be five times as large as is necessary. It is directly proved by the experience of London and Providence that about thirty gallons per day per human being is ample to supply all real needs; but in consequence of the system generally adopted by American water corporations, which put a price per year to consumers and allow them to draw all the water they choose, the quantity per person has steadily risen until it has reached, in some cities, the incredible quantity of 150 gallons per day.

Time after time, in many cities, the public works have been doubled to cope with this increasing demand, but their limits have soon been reached, until water commissioners, in despair, have now seriously sounded the alarm. The public conscience has been appealed to, detectives and police have been sent from house to house in Chicago and other places, and fines and penalties have been inflicted to stop this waste, but all to no purpose. Every water report puts the waste at, at least, sixty per cent.

The twenty-sixth annual report of the Board of Water Commissioners of the city of Hartford, in which it states that the average daily amount of water used and wasted in Hartford is equivalent to over one hundred gallons to each and every person—a quantity which no city in Europe approaches, and which is only equaled by two or three in our own country; that the cause of this waste was permitting the water to run in cold

weather to keep the pipes from freezing; in summer, letting the water run to cool it; from extravagant use of hose and lawn sprinklers during the hours prohibited by the rules, and at all times from water closets.

The report states that it is impracticable to use water meters until one is invented which combines cheapness and a fair percentage of accuracy and durability. The same report shows the result of an investigation of this waste in the case of the average dwelling-house in St. Louis, occupied by a family of six persons. The amount of water was measured, when used in the ordinary way, and found to vary from 1,310 to 1,903 gallons per day. The amount consumed was then measured, when care was taken that there should be no waste in the closet, and found to be 758 gallons per day. Subsequently the water was measured, when, as stated by a member of the family, a very free use of water was made, only ordinary care being taken to prevent its wasting, and the amount consumed was found to vary from 433 to 464 gallons per day. On the other hand, one day an account was kept of the amount actually consumed for useful purposes, and it was 178 gallons.

The evil of enormous waste is not one of mere dollars and cents, for water works are depended upon against great conflagrations. But, with the present distributive pipes in the streets, we cannot let this waste continue and still maintain an effective fire pressure for hydrants, even though we had an indefinite quantity in our reservoirs. The pipes are too small so long as everybody is drawing *ad libitum* from them. You cannot play streams forty feet high from the hydrants in many parts of this city.

The hotels and large manufactories use enormous quantities of water, mainly legitimately, but if thousands of private users are running three gallons to waste for every one gallon really used, these hotels and manufacturers are unjustly compelled to pay more than double what water ought to cost, and more than double what everybody else is paying.

The cause of all this is that city councils, in selling water to the community, do not make each person pay alike for the quantity used, and at the cheapest rate, and prevent him from getting more than others are entitled to who pay the same.

Apply to gas the same system that is applied to water, and you would bankrupt every gas company in existence. Many people would never trouble themselves to turn off the gas, but let it burn, if it costs no more whether it burns or not.

The remedy for existing abuses is to be found in making users responsible by measuring the water used through proper water meters. Then, if they wish to waste it, let them pay for it. The result would be to cut down the waste of sixty per cent., and this would be equivalent to doubling the water works.

In answer to the objection sometimes urged against reducing the supply of water to a reasonable basis, "that we must let the water run continually in many cheap buildings to prevent freezing of the pipes," he said that when men put up shambling tenements to make a large return upon a small outlay, it is unjust to force the rest of the community to pay for the tenement man's meanness.

The constant cry of the demagogue who calls himself a practical man is, "Don't stint the poor man!" But I do not wish to stint any one. Ascertain how much is actually needed, and then double it, but stop the waste somewhere.

He advocated the plan of having the city put one or more main meters on each house, and then let the owner of a tenement house put one upon each tenant, and said that there are always a few difficulties in the way of any improvement, but they disappear before the light of experience.

The only important argument against the adoption of a general water-meter system has hitherto been that no meter has been found sufficiently reliable under all circumstances to be depended upon. This has been, in the main, true, as proved by experience.

In Providence, where water meters are used, it is found that thirty per cent. of them must be repaired every year, and that the coming meter has not yet arrived.

Water meters in use, up to this time, are constructed upon two principles—the piston and the rotary; but in both cases we are trying to make a tight vessel in which to measure water by the mere con-

tact of two pieces of metal in movement against each other. In both cases the impinging or sliding of two surfaces of metals against each other is involved, and when two surfaces of metals rub together, especially if there be mud or grit between them, as is liable to be the case in water meters, they wear leaky.

It is not practicable to remedy this by means of nicely-adjusted springs and rings which require skill to keep them in order, as a water meter must be left to itself in exposed situations; hence, the entire system of piston and rotary meters is fundamentally wrong in principle.

He next proceeded to sum up the requisites for a water meter, stating that they should be:

First. It must not wear or corrode, so as to allow water to pass through it unregistered.

Second. Its action must not be affected by mud—a terrible element for water meters.

Third. It should not let water that has once passed through it into the house pipes return again to the street mains, to the loss of the consumer. This is a fault with nearly, if not quite all meters in use. You can see how it affects the consumer, say in New York, and even in this city in certain localities, where, after ten o'clock in the morning, when everybody is drawing, you cannot get water above the second story of the buildings in such district. At night the water mounts higher to fill the pipes, and is registered, then descends and remounts, and is registered with every variation in pressure; consequently, a certain large percentage of water is registered over and over again.

Fourth. A water meter should have no stuffing boxes or gearing to wear out and get leaky, nor springs or cranks which corrode and get out of order.

Fifth. It should not make objectionable noise or produce concussion in the pipes, as the pipes, when suffering themselves from constant shocks, also conduct the noise over the house.

Sixth. It should be able to withstand the rudest shocks and violent changes.

Seventh. A water meter should present but the smallest obstruction to the flow of water. There are many meters in use which reduce the flow of water from ten to forty-five per cent.

Eighth. It should deliver water with a smooth and even flow—an absolute condition where fountains or motors are desired.

Ninth. The expense for maintenance must be trifling.

Tenth. The parts must be simple, durable and cheap.

Of the hundreds of attempts to produce a good water meter, no more than half a dozen have been found to approach in practice anything like success, and only two or three have been found by water boards to be worthy of adoption. But the city of Providence finds that thirty per cent. of all the meters were taken out and repaired during the year, and the Chicago report says that one thousand piston meters cost $17,000 for repairs in nine months' time, thus showing that the best types of meters thus far employed were unsatisfactory in durability, requiring great expense for repairs, and causing great annoyance to consumers by interruption of supplies. They have also been very inaccurate, over-registering and under-registering under various pressures.

Mr. Coleman next explained the reasons why these imperfections should be expected in piston or rotary meters, and then said:

The true principle upon which a real water meter depends seems to me to be contained in a quart pot. It is a tight vessel; you fill it and empty it, refill and empty, and there you have an exact measure. If you have an india-rubber bag, and fill and empty it, you have the same principle of exact measurement.

The Spooner diaphragm meter is constructed on this principle. It is formed of two chambers, the upper one containing the valve mechanism, and the lower one actuating the diaphragm and discs. The valve shaft, which passes through the valve chest, carries three valves, the center one being double faced. The valve chest is divided into three compartments, with four parts; thus, at each movement of the valve shaft, two ports are closed and two are opened, admitting the water to the measuring chamber on one side of the diaphragm, and allowing the water on the opposite side of the diaphragm to pass out of the meter. The lower or measuring chamber is divided at the center by a diaphragm of india rub-

ber, moulded into concavo-convexed form.

The edge of the diaphragm makes the packing between the two castings forming the chamber. On each side of the diaphragm there is a perforated disc, with the edges curved backward, so that all wear of the diaphragm against the disc is prevented. On the back of each disc there is a projection which rests on a stud, which is fastened to the shell of the meter, the disc sliding forward and back, moving in its action the lower end of the levers.

On the outside of the casting forming the upper chamber is placed the registering mechanism, actuated by one end of a lever that enters a recess in a horizontal moving bar; the other end of a lever enters the chamber, and is worked by the moving parts of the meter. The water enters from the supply pipe into the upper compartment, and passes thence through an open port to, say, the right-hand side of the diaphragm, which it moves slowly towards the left disc, forcing it against the lower end of the valve lever, thereby reversing the position of the valves and changing the flow of water to the other side of the diaphragm, when the operation of the moving parts of the meter exactly reverses. While the water is passing into the measuring chamber on one side, precisely the same quantity of water is being discharged from the opposite side of the diaphragm, the flow being smooth and without interruption. The meter discharges a uniform measure of water at each movement of the diaphragm under any variation of pressure.

Mr. Coleman claimed that this meter possessed the requisites for a water meter which he had already enumerated, and then said: It may be proper to say that my attention was called to this meter on my return to this country last autumn, and a request made that I should examine it professionally as a piece of mechanism. I did so, but insisted upon making a series of trials before giving a report upon its merits. Through the kindness of the Water Board of Boston, we gave it long and exhaustive trials; and subjected it, among others, to the following unusually severe tests:

1. The rapid opening and shutting of the supply cocks under a full head of water made no difference in its accuracy.

2. The water was permitted to drop slowly from the outlet for fifteen hours, and at the end of that time we found six cubic feet of water in the tanks, and six cubic feet were registered on the dial. The Water Department reported a variation of about two per cent. under a very small flow, but this is readily accounted for by the air contained in the water.

In addition he presented the opinions of other water engineers in its favor, and then said: If we have succeeded in presenting any arguments which have convinced you that the water-meter system is the proper method of selling water, I trust you will believe, as I do, that the meter invented by Mr. Spooner is an instrument upon which municipal corporations may safely rely for accuracy and thorough durability, as well as for all of the good qualities which are indispensable in a water meter.

In answer to certain questions, Mr. Coleman said that the diaphragm is composed of pure rubber without any fabric, and hence is very durable. Any mud or sand that might accumulate is washed off by the water, since the diaphragm and the valves are vertical. The points that have to exert thrust are bushed with hard rubber and brass to prevent rust from blocking up the joints. They have been carefully testing it thus far, wishing to be sure that it was accurate and durable before asking corporations to adopt it, and the last patents were secured only four or five months ago; but the tests to which the meters have been subjected have been of extraordinary severity. He also stated that one of the meters constructed during the experimental stage of the invention has been in constant and successful use in Syracuse, N. Y., during the last six years.

Recently, says the *Engineering*, the firm of Sir W. Armstrong & Co. has submitted for trial a breech-loading gun having a peculiar construction. The whole of the piece in rear of the trunnions is built up of steel wire, over which is shrunk ordinary yet thinner coils of great tenacity. It is said to be capable of bearing an explosion of 300 lbs. of slow-burning service powder. Although the weight of the gun is only 21 tons 4 hundredweight, it has a bore of 10.238 inches.

DISCUSSION ON THE ANALYSIS OF POTABLE WATER.

By CHARLES WATSON FOLKARD, Associate Royal School of Mines.

From Proceedings of the Institution of Civil Engineers.

II.

DISCUSSION.

Dr. TIDY said, in discussing the question of water supply, it was important to grasp its many-sidedness. When it was desired to supply water to a town, various possible sources were selected, and samples were sent to a chemist, whose duty it was to analyze them. It was not for the chemist, however, to say whether the water was pure or impure. To him, pure water was hydrogen and oxygen, nothing else. To him, 1 cubic inch of dissolved gas, or 1 grain of dissolved matter, were impurities. The chemist had only to say what was the composition of the water submitted. From the chemist it passed to the sanitarian, the medical man, whose view of the subject was essentially different from that of the chemist. With the analysis in his hand, he had to ask himself if the water was likely to be a proper one for the supply of the town for which it was proposed. He could not experiment with the water, but he endeavored to ascertain where waters of a similar kind had been supplied, and what had been the result. That was the medical aspect of the question. It then passed to the engineer. It having been decided that the water was good, the engineer asked himself, "Is there sufficient to supply the town, and are the conditions such that it can be delivered at a moderate cost?" That was the engineering aspect of the question. It was essential to his purpose to separate these three. In criticising the paper, perhaps somewhat severely, he might be permitted to say that he had had some experience in water analysis. Without reference to the time during which he had been in practice for himself, he had, during the many years that he had assisted the late Dr. Letheby, made nearly four thousand analyses of water with his own hands; and as a medical man he had also had something to do with the sanitary aspects of the question. He would not discuss the various processes of water analysis, which he had himself dealt with at considerable length elsewhere. The author had stated that chemists were "powerless to help the sanitarian in discriminating between wholesome and unwholesome water." Dr. Tidy did not pretend to say that the chemist could do everything, but he maintained that, given a reliable analysis of water, the chemist, or rather the sanitarian, was able to speak with almost unhesitating certainty in bringing it to bear on the sanitary question. What were the means by which to arrive at a true chemical knowledge of the composition and properties of water? He admitted, with the author, that the varieties of organic matter in potable water were somewhat numerous; chemists therefore, did not conduct a water analysis with the same certainty as they did a quantitative analysis of a body, with the exact constitution and composition of which they were familiar; but considering that two out of the four processes described in the paper, vastly different as they were in their action, closely agreed in their results, he thought the public might reasonably have some faith in these as a means for estimating the organic matter in potable water. As he had shown before the Chemical Society, with reference to nearly two thousand cases of water analysis treated by the combustion process of Dr. Frankland, and by what Dr. Tidy had called the oxygen and others the permanganate process, the actual results were as nearly as possible identical. A report would shortly be issued by himself, Dr. Odling, and Mr. Crookes, on London water. No fewer than three hundred waters had been examined by both these processes, and by means of a series of wave diagrams it would be shown how closely they agreed in the story they had to tell. The author's statement that the chemist was powerless to help the sanatarian was a very strange one, coming from a chemist. What were the reasons he assigned for this power-

lessness? In the first place he stated that "it is an ascertained fact, proved beyond possibility of doubt, that mere dilution, how far soever it be carried, does not render inoperative the specific action of living germs" (p. 11). His second reason was that "the germs which cause or accompany disease are endowed with the most persistent vitality, and are capable of withstanding heat, cold, moisture, drought, and even chemical agents, to a marvelous extent" (p. 12). That was all very well, but where were the germs? In only three diseases, pig-typhoid, remittent fever, and splenic fever, had anything of that nature been detected. No such thing as a typhoid germ had been discovered. One could no more analyze a water for the germ of typhoid, than one could analyze the brain for an idea. Not only, however, did the author speak of germs as though they were tangible, but he had fixed the conditions of the life of a thing the very existence of which had never been proved. As to wholesomeness, the author expressed his belief that the only safe test was by tracing the water to its source. What source? He doubted whether there was a particle of water in creation that had not passed through an animal body once or more. For himself, looking at the subject as a medical man and as a chemist, he believed the true test was not what the water was, miles off, but what it was at the place at which it was proposed to be taken for supply. That was the practical method of testing it, and it was a method always adopted in other matters. Engineers should not trouble themselves about what the water was 50 miles off, or fifty years ago, but consider what it was at the time and the place where it was proposed to take it. The author, naturally, with his views, condemned all rivers. He did not mince the matter, but said, "This will at once condemn all rivers flowing through a populous country" (p. 12). And he added, by way of illustration, "Take, for example, the case of a river with a town of 50,000 inhabitants on its banks. If supplied with water at high pressure and sewered, the amount of foul water discharged into the river will be about 1,000,000 gallons daily, irrespective of the rain-fall, which will bring with it the washings of the streets, &c. Taking the total flow of the river at 500,000,000 gallons, and supposing that the water is perfectly pure when it reaches the town, there will be a mixture of 1 part of sewage in 500 parts of clean water, for the inhabitants of the next town to drink. Take now an infected liquid and add 1 part to 500, or even to 500,000 parts of liquid susceptible of infection. The mixture will swarm with lop organisms and become putrid in few days, provided only the conditions are favorable" (p. 13). Then he asked, "What may be expected to happen to the unfortunate inhabitants of the lower town? Simply this, that the strong and healthy will have sufficient vitality to throw off the poison, but the weak and sickly will succumb, inoculated by the dejecta of zymotic patients in the upper town." "The above," said the author, "is no fanciful picture." Fanciful was not the word for it, and he hardly knew a word to express it, but certainly a more far-fetched picture, a more unbridled effort of the imagination, he had never come across. He wished to ask the author to explain how it was that, in the case of towns affected with cholera on the banks of rivers, having regard to the period at which the outbreak of cholera occurred in those towns, the disease had invariably gone up the river and not down. He challenged the author to produce a case in which the passage of cholera had been without a break down a river. The only case given in the paper of injury from river water was one in which the experiment of drinking polluted water had been tried on the inhabitants of a town in Surrey. He thought he knew the town to which the author referred, and if he was right in his presumption, the case was one in which he had been himself consulted professionally, and he believed also Dr. Frankland. They had both written a report, and he was prepared to show if necessary, that the illustration in question had nothing whatever to do with the subject. The author had further stated that there was not the least evidence to show that foul water was rendered wholesome by flowing 50 or 100 miles. Dr. Tidy maintained that a distance of 10 miles was sufficient for the self-purification of water under proper conditions. A few weeks ago Dr. Dupré and himself

had seen a wonderful illustration of the self-purification of water within a very much shorter distance. Turning to the sanitary aspect of the question, he would remind the members that in England there was a large number of towns supplied with well water, and a large number supplied with river water. He had taken the death statistics for ten years of thirty-six of the largest towns in England, eighteen being supplied by deep well water, and eighteen by river water. The eighteen towns supplied by well water had a population of 889,340, and the eighteen towns supplied by river water had a population of 911,742. The average death rate of the towns supplied by wells was 22.72 per thousand, and the average death rate of the towns supplied by river water was 22.66 per thousand. In fever and some other diseases there was (except in certain cases that could have nothing to do with the water) a decided advantage on the side of rivers. It might be said that he had taken a number of towns indiscriminately and mixed them up together. To meet that observation he had examined the death statistics of London, as Mr. Baldwin Latham had done. He had gone carefully over Mr. Latham's figures, brought them down to the latest date, and elaborated them somewhat more fully. London was supplied by eight companies, five of which derived their supply from the Thames, one from the Lee entirely, and one from the Lee and from wells (the New River Company), and lastly, one that derived its supply exclusively from deep wells in the Chalk. The death rate for ten years of parts supplied by river water was 21.57, whilst that of the places supplied by deep chalk wells was 21.48. He had gone through the various diseases, and had found that while certain diseases, such as croup (which he thought could scarcely be traced to water), appeared to be a little more prevalent in the river districts, certain other zymotic diseases were somewhat in excess in the districts supplied by wells. It had been proved before the Duke of Richmond's Commission by the experiments of Dr. Frankland and Dr. Odling jointly, and these experiments had been since repeated, that at Hampton the river contained if anything less organic matter than the water at Lechlade, where the Thames first assumed the condition of a river. That water purified itself in a running river he was as certain of as he was of his own existence. And this self-purification was effected first by the process of subsidence, the solid matter in the water being carried down; secondly, by the process of oxidation (the oxygen being partly derived, no doubt, from the air, and partly from plant life); thirdly, by the action of fish. He had no doubt upon that point, and he spoke with a knowledge of many of the important rivers in England and Ireland. In conclusion, he desired to ask the author a few questions. First, admitting the complexity of the organic matter in potable water, and that the true test of the value of different processes for its estimation was consistency in their results, had the author ever attempted to prove or disprove such consistency; and, if so, could he favor the institution with the details of those experiments? Secondly, admitting his theory of rivers being such important agents in spreading disease, would he explain how it was that in outbreaks of cholera where towns had been affected along the banks of a river, the order of attack had been invariably up the river, and not down? Thirdly, would he explain, in view of his alarming picture, how it was that towns supplied with river water showed no greater general or zymotic death rate than towns supplied with deep well water; or if he stated that which was not true, would he bring forward facts to contradict it? Would he explain, further, how it was that in London the parts supplied by the Kent Water Company showed an almost identical general and zymotic death rate with those supplied by the waters of the Thames and the Lee? Fourthly, admitting that there might be germs in running water, could he adduce any evidence to show that under natural conditions of flow and contact with oxygen they were not amenable to the same laws as organic matter generally? He would only say that if the chemist desired to gain the respect of the engineer or of the sanitarian, he must not indulge in far-fetched and fanciful theories or hypotheses, but confine himself strictly to the arena of facts.

Dr. THUDICHUM said when important

questions were concerned, and one had a strong conviction to state, it was not easy to find a form in which to make that conviction acceptable. Nevertheless, he hoped to make himself intelligible on some of the main points which he desired to illustrate. He congratulated the author on having made on the whole a clear, succinct, and practical statement. No doubt it required on his part a great deal of courage as a chemist to come forward and tell his brother chemists that they were groping in the dark, and that their analyses were valueless. If chemical analyses of waters were to be discredited, Dr. Thudichum would feel much regret; but there was a great deal of truth in what the author had said. It had been stated by Dr. Tidy that he had latterly come to the conviction that Dr. Frankland's analysis of water was as good as his own. If the members had been present at the meetings of the Chemical Society, when that matter was discussed, they could hardly have believed what had since taken place. Neither having convinced the other as to the uselessness of his particular mode of analysis, they at last became friends, and said to each other, "Your analysis is as good as mine; let us embrace and be friends." What did those analyses mean? They ascertained that a certain amount of organic matter was present in water intended to be drunk, but they showed no more. The organic matter, for example, contained in Thames water could not be shown to be noxious to health. Chemists had not shown at what particular concurrence of conditions they were to begin to consider water injurious which contained a certain amount of organic matter, and under what circumstances it was to be considered wholesome. Waters taken from sources like rivers always contained organic matter, because they were always flowing over large surfaces clothed by vegetation, living or dead, and under all circumstances there was a certain amount of dead, organic, vegetable matter present in watercourses. How innocent the organic matter of the river Thames was he had proved in this way. He had sent to the places where the water companies took their water, and caused to be collected a large amount of organic matter, carried it to his laboratory, infused it with distilled water, and allowed it to stand a certain number of hours. He then analyzed it, and found what he expected, that this distilled water had assumed, with regard to organic matter, the properties of Thames water. He therefore maintained that the analysis of water, with reference to the quantity of organic matter contained in it was, hygienically speaking, of no value. The next point to which he desired to refer was the bearing of the results of biological and microscopic research on the subject under consideration. That led to the point on which the whole argument oscillated. Under what circumstances was water wholesome, and under what circumstances was it unwholesome? There might be waters which contained so much inorganic matter as to cause diarrhœa, but such waters would be so unpalatable that they would not be drunk. On the other hand, there might be waters perfectly clear and palatable in which the chemist would discover no appreciable amount of organic matter, and yet they would carry death whereever they were consumed. That was the biological aspect of the question, and in regard to that aspect microscopic art was just as impotent as chemical art to determine whether water was wholesome or not. Then what test could be applied to ascertain the fact? There were various tests, some of which had been unpremeditated. For example, when in the East of London cholera swept along the river Lee and attacked twenty thousand persons, that was an experiment on a large scale. When again in the South of London two companies rivaled each other which should proceed in the most successful way to distribute cholera amongst their consumers, as in 1848 and 1854, other examples were made on a large scale. If another example was required, showing how water might be contaminated without microscopists discovering it, the case of the poisoning of Caterham Well might be taken, by means of which three hundred and fifty-two persons contracted typhoid fever, because a small amount of excrement from a sick person who was allowed to work in the well got mixed in the water. Under such circumstances it was necessary to see with an eye which was not microscopic, and to apply a certain argument

which was not chemical, but which was hygienic or medical. Water might be bright and brilliant, and yet contain the germs of death in it. It was well known that things might have organs and a certain chemical composition, and yet not be visible to the eye. Take the case of a minute drop of blood; put it on a microscopic slide, and add water to it. All the corpuscles were before seen to be red, and their shapes were distinguishable, but after the addition of the water the coloring matter was withdrawn, and no power of the microscope could make them visible. Here was a case in which an organized body of the diameter of $\frac{5}{1000}$ of a milimeter could be rendered invisible, and how much more might that be the case with a body having perhaps not $\frac{5}{1000}$ part of the diameter of a blood corpuscle? He referred to those germs which in the last thirty years had been proved to exist as the causes of zymotic diseases. He would refer, as an illustration, to the germ of the fowl-cholera. It was as distinct a germ as could be made out, visible under the microscope, having spores, still minuter particles, which were to the bacterium as the seed was to the plant. If those germs were preserved for a certain time in a closed tube, a cloud would at first be seen, but as the oxygen in the tube was removed and consumed, the germs assumed a different shape and appearance; they were lost to sight altogether. How were they to be found out? Not by the microscope, not by chemistry, but by taking a needle and dipping it into the liquid, which was perfectly transparent, and then inserting it in the cutaneous tissue of the fowl, and in a few days the fowl would be dead. It was impossible to experimentalize with water merely, so as to show whether it was wholesome or not. What then followed? What hygienists had always maintained, that water should be taken from natural sources which were neither contaminated nor contaminable, and those should be the only sources of drinking water for communities and individuals. Could this proposal be carried out? Of course it could. In the neighborhood of London, for example, taking a circuit of 30 miles, 100,000,000 gallons of spring water could be found running every day, which would be amply sufficient to supply the culinary and drinking wants of London. In the neighborhood of Hertford, for instance, there was a spring yielding 10,000,000 gallons a day. It ran into the river Lee, and there would be no practical difficulty in taking it out of the river, and sending it direct to London, without allowing it to be contaminated by dung-boats and all the filth that accumulated in the river. The citizens of London, who first attempted to supply the city with water, did not go for river water, but for spring water, and it was for the conduction of spring water to London that they got their first Act of Parliament. In like manner, engineers should set about it now, everywhere getting all the spring water they could to supply towns. They would find in every neighborhood a sufficient supply to satisfy the public wants. London, of course, would require a double supply, according to the proposal worked out by Sir Joseph Bazalgette, Mr. Easton, and Sir F. J. Bramwell, a proposal which had his greatest admiration. It should not be imagined that because it was strange it was unparalleled. In fact an example might be found in a town having much more limited means than London. He held in his hand a report by the Government of Würtemberg on the public water-supply of that kingdom, a kingdom which he believed was at the head of civilization in regard to that question. In the capital, Stuttgardt, there were two supplies, one of common water for watering the streets, filling baths, and flushing closets, and another for drinking and cooking. Numerous instances might be cited from that report of the care taken to supply even the lowest classes of the community. Even the villages on the highest mountains in the Raue Alb were supplied with excellent spring water, to the extent of 60 liters per head per day. It was pumped to the height of 310 meters, and the pressure in the pipes was 75 atmospheres. If a small village of that kind could be supplied with pure spring water, would not the richest town of the richest nation in the world be able to get the same security against disease? The dangers threatening were very great. Perhaps not once in ten years would a river carry disease massively in its water, but if it did so once in a century it should be provided

against. The water from the downs of Hampshire came filtered through hundreds of feet of chalk. It was of the greatest purity, cool, and having no organic contamination of any kind, and if it were taken through pipes to the consumer in London, under a system of constant supply, all danger would vanish; but if the towns continued to be supplied with water from rivers, there would certainly be, on some occasion or other, a failure of filtration, the introduction of disease, and a repetition ofthe fearful and melancholy lessons of the last thirty years, during which one hundred thousand people had been crippled, and not less than twenty thousand had died from poisoned water. With the qualifications he had mentioned he had fully agreed with the author, and thanked him for having afforded an opportunity of discussing so important a question.

Mr. Homersham said for more than thirty years he had been in frequent communication year by year, with analytical chemists and microscopists in respect to the examination of water from different sources, to make selections for the supply of water for drinking and domestic uses. Many of those men, some of them personal and intimate friends of his own, as Clark, Graham, Lankester, Miller, Newport, Ronalds, Thomson, and Ure, were no more. From frequent communication with these, and still more frequent communication with others who remained, and from experience gained in designing and carrying out various works for the supply of different towns and places with water for domestic use, not only in the United Kingdom, but on the Continent of Europe, and places more distant, he was pretty familiar with what had been urged for and against waters derived from different sources. He made that statement to ask for indulgence, in case he should appear to speak somewhat dogmatically. With regard to the paper, it appeared to him that the word "previous" in the title had been unnecessarily added. For practical purposes, the point to be determined was the amount and the quality of sewage or other present injurious contamination, if any, in water for potable and domestic uses. Such water should be (1) at all seasons clear, transparent, bright, and, when seen in large bulk, pure blue, that being the natural color of uncontaminated water; (2) well aerated, holding in solution from 7 to 8 cubic inches of air per gallon, consisting of 2 or more cubic inches of oxygen and 6 of nitrogen; (3) it should have at its source a uniform temperature equal to the average of the climate for the year, which in this country varied but little from 50° Fahrenheit; (4) should be free from living organisms, vegetable and animal, and from all dead decomposing organic matter, and should not dissolve lead; (5) should hold only a moderate quantity of mineral matter in solution, and thus be soft and not deposit a coating of lime or magnesia when being boiled. On the subject of potable water, he thought it was very questionable whether many persons drank cold water from choice. Where it was drunk at all, it was among the lower classes who unfortunately could not help themselves. When boiled it was drunk to a large extent, as in tea and coffee, and it was very largely used in culinary operations, and it was important that water used for such purposes should be such as did not deposit fur in boilers or tea-kettles. Uncontaminated spring- or other water, derived from a considerable depth below the surface of the earth, was the only water that at its source had a normal even temperature at all seasons, summer and winter, and, as far as he knew, was also free from living organisms, vegetable and animal. It was also difficult to find any water but spring or subterranean that was at all seasons clear, transparent, bright, and when seen in large bulk, blue. Water derived from brooks or rivers, or from lakes, natural or artificial, varied in temperature at different seasons of the year, being comparatively warm in summer and cold in winter; it was more or less opaque, and when seen in bulk lacked the blue color peculiar to uncontaminated spring-water; it had in solution in warm weather less oxygen gas than spring-water; it held partly in suspension and partly in solution, after rains in hot seasons, manure washed from land and droppings from animals; and it also abounded in life, vegetable and animal, and was liable to inoculation by means of drains with the virus of specific diseases, causing ill-health and often

death to those who drank it. He agreed with the author in thinking that when samples of water from different sources were submitted to mere chemical analyses, it frequently happened that the results gave very little clue to their wholesomeness, or the contrary. He said very little clue, because there could be no doubt that chemical analyses often did give some clue, but in other cases it gave none whatever. Chemical, and only chemical, analysis could be relied upon to determine the quantity and quality of the gaseous contents of the water, the mineral contents and consequent hardness. The brightness, color and transparency of the water could be judged by the sight. Chemistry threw little light upon the nature, quantity, and quality of the organic matter that might be dissolved or mixed or lived in waters. Supposing, and this was common with river, lake and other surface waters, a water to contain a large quantity of minute organisms, say several species of living plants and animals, and several hundreds of each species in half a gallon, the chemist boiled all those plants and animals with the water, and after evaporating the liquid he weighed the residue, and then subjected it to a process of cremation. As the small animals and plants were composed of more than 90 per cent. of water, the loss in weight of the residue after cremation must be multiplied by 10 at least to arrive at their weight when alive. As to the names, or peculiar forms or qualities, wholesomeness or unwholesomeness, of the plants and animals, chemistry, to use the words of the author of the paper, was "powerless to help the sanitarian." Knowing that, it had been his practice during the last thirty years to submit samples of water, not only to an analytical chemist, and thus obtain all the assistance that could be had from chemical science, but to submit also samples to a competent microscopist and medical man well acquainted with the forms, names, habits, and other properties of the animal and vegetable organisms pervading many waters. The practical importance of such microscopical examination would be evident from the following considerations. It had been well established that when certain microscopical plants of the nature of bacteria pervaded a water, to drink such water often gave rise to remittent fever, splenic fever, and pig typhoid. Chemistry was unable to discover these microscopic plants; but a competent medical practitioner acquainted with the properties and habits of those minute organisms could detect at least many of them and others of different kinds. In June, 1852, both the late Dr. E. Lankester and Dr. Redfern, the present professor of anatomy and physiology in Queen's College, Belfast, found from thirty-two to thirty-eight species of microscopic organisms, some plants, some animals, and some diatomaceæ, besides large numbers of each species in half a gallon of water, drawn direct from the supply pipes of the Lambeth Company (taking its supply at Thames Ditton), before entering any house cistern. In 1857 Dr. Hassall, in a report to the then President of the General Board of Health, stated that any water drawn direct from the mains of each of the waterworks under the provisions of the Metropolis Water Act, 1850, still contained considerable numbers of living vegetable and animal productions belonging to different orders, genera and species, but especially to the order or tribes annelidæ, entomostraceæ, infusoriæ, conferveæ, desmideæ, diatomaceæ, and fungi. Dr. Hassall stated that the examination was made in winter, and that other examinations should be made in spring, summer and autumn. No such further examinations, however, had been made by order of the Government. That, he thought, was a great dereliction of duty on the part of some department. Winter, it was suggested, was not the time to find the plants so well as summer and autumn, yet no other authorized examination had been made. The waters of the various companies were subject only to chemical examination. In the last Report of the Government Water Examiner under the Metropolis Water Act, 1871, a chemical analysis was given by Dr. Frankland, another by Messrs. Wanklyn and Cooper, and another by Drs. Bernays and Tidy. In that report, there was no mention of microscopical examination. If microscopists were employed to examine the water month by month they would find out the species that were more frequent at one season than another, and ascertain in what water they

abounded. It was well known by those who had paid attention to the subject, that many classes of those plants and animals indicated unwholesome water, and that these were mostly to be found in warm weather. It was true that Dr. Frankland, with his analyses, reported that the Grand Junction Company's water contained moving organisms, but no particulars were given; while in the reports of Messrs. Wanklyn and Cooper and of Drs. Bernays and Tidy the presence of any organisms was ignored. That reminded him that only the other day a shareholder who wrote in the *Times* newspaper stated that the company was satisfied with the report of its chemists, because they did not mention any living organisms; but it was not because there were none, but because no microscopists had been employed to detect them. Surely if it was worth while to have the companies' waters chemically analyzed once per month by five professors of chemistry, it should be made a point to have at least one examination of the waters in a month by a competent biologist and microscopist. In obtaining samples of water from distributing pipes for determination of the organic contents, the water to be examined should be drawn not only direct from a main but near to the "dead end," as it was technically called, of a rider pipe, or to the dead end of a service main placed in a side street, for the organisms existed in much larger quantities near the dead ends of mains than in circulating mains. The creatures were so intelligent that where they found the water quiet they went to live and breed. Chemists sometimes asserted that water had not been properly filtered. Filtration in some respects really injured the water in summer, because during the process there was collected on the top of the sand a further quantity of organic matter that became decomposed, and furnished pabulum for the insects. The author had stated that reservoir- or lake-water contained but a small quantity of organic matter, but he did not agree with that statement. It would be found by the Registrar-General's Returns that wherever lake-water was supplied to a town there was an excessive mortality. But, putting that aside, as there were many other things to cause mortality besides impure water, yet such things as the excreta of animals, liquid and solid, leaves and the like were unavoidably washed into the water. Water contamination in lakes also arose from the formation of mud on their unlined sides and bottoms. It was impossible to prevent the formation of this mud, which was congenial to the production and growth of animal and vegetable life. The water from Loch Katrine and the water supplied to Manchester were full of dead organic matter and living organisms, especially in the summer. The author had further stated that very slight contamination took place in water when exposed in the open country; but he could not agree with that statement. He remembered having a large reservoir lined with cement on the South Downs, for the supply of Brighton. The water was perfectly pure when pumped from the wells and into the open clean reservoir, but in a few hours in the summer, there were masses of confervæ growing on the top of the water, and soon after a number of insects of different orders bred and flourished in it. It was a serious expense even to clear out the reservoirs and keep them clean in the summer. The evil could not be prevented except by roofing them over. Carbonic acid was given off from bicarbonate of lime, which formed the pabulum that the spores of the confervæ required, and the consequence was the water was polluted though the open reservoirs were in the country. He had seen open reservoirs in a hot day when clouds of insects had been blown by the atmosphere into and upon the water in heaps. It was an entire mistake to suppose that water could be kept pure in an open lake or reservoir because it happened to be in the country. The temperature of the Thames in a hot summer was as high as 72°, and in the winter it was as low as 35°. Water, when it was warm, lost some of its oxygen, and plants and animalcules bred in it to a much larger extent than when it was cold. The loss of heat in winter, bringing the water down to within 3° of freezing point, rendered it liable to freeze readily in the consumer's pipes, and thus burst them. There was another point on which he disagreed with the author, that water to be purified must un-

dergo a process of distillation by the heat of the sun. Water that fell on uplands composed of porous strata, such as sandstone, chalk, &c., was absorbed and percolated downwards often to great depths through the pores of the strata. A quantity of water was held in the pores by capillary attraction, and diffused through its mass. The varying density of the air brought the water thus held by capillary attraction in contact with changed oxygen, and by that process long-continued deprived the water of any organic matter it might have possessed. Supposing a depth of 18 inches of rain to go down through the surface in the course of a year, as the chalk strata were on an average more than 600 feet in thickness, and one-third of the bulk consisted of pores, it followed that it would require a depth of at least 200 feet of rain, or the produce of one hundred and thirty years, to saturate the pores.

Professor TYNDALL observed that Mr. Homersham had had very valuable experience in regard to the subject under consideration. He had gone with Mr. Homersham to Canterbury, and seen the chalk-water there, and the mode of softening the water according to Clark's process. He did not know that he had ever seen a more beautiful experiment upon a large scale. He had also seen the same thing at the Chiltern Hills and at Caterham, where the works were under the supervision of Mr. Homersham. There was one point, however, in which he was inclined to differ from him, and to agree with previous speakers. He was rather doubtful as to the ability of a microscopist, even though he were a medical practitioner, to detect in water the germs that were chiefly damaging to man. He would take the case referred to by Dr. Thudichum, and a more lucid medical investigation he had never known. There was an outbreak of typhoid fever at Red hill and Reigate, where more than three hundred persons were attacked. Dr. Thorne went there, got hold of the tag-ends of his facts, fitted them together, traced them backwards, and finally came with the utmost certainty to a single individual who had been employed in sinking the well at Caterham, and whose excreta had infected the whole neighborhood. Imagine the diffusion of the infective matter through all those long pipes, and a medical practitioner trying with his microscope to find out the little infected particles. In his opinion it would be a hopeless task. In the case of that most virulent disease, splenic fever, which had been worked at so successfully by Pasteur, the germ was easily seen. It was a large bacterium. But there were bacteria that were not easily seen. He had, for instance, a cascade near a little house on the Alps, 7,000 feet above the sea, and although it was charged with water coming from the snow-fields of the Alps, if he took a speck of that clear water and infected an organic infusion with it, in forty-eight hours the infusion would become putrid and swarming with organisms. He once chose a piece of the clearest ice he could find, placed it under the receiver of an air-pump with perfectly moteless air around it, and allowed it by fusion to wash its own surface. From the heart of that ice, clear as crystal, he took a quantity of water, and gave it to Dr. Burdon Sanderson, who found that it contained germs of bacteria just as effective in producing putrefacation as ordinary water. He should not, therefore, like to accept the notion that germs were so easily detected by the microscope. He agreed with Dr. Thudichum, that chemical analysis would afford but little information as to the deadliest things that might be in water, and that the microscopist could tell very little about them; but that the best way was to draw water supplies from sources where contamination could not come into play, and in that respect he desired to say that Mr. Homersham stood conspicuous among engineers.

Mr. JABEZ HOGG remarked that, as a microscopist of some experience he agreed in part with what had fallen from Professor Tyndall as to what the microscope could do, and what it could not do. He admitted that the microscope had never disclosed the kind of bacterium that would produce a specific form of disease, but he could not agree with him that the microscope could not detect the presence of bacteria. It could not perhaps detect the exact formation of the creature moving under the field of the microscope; but microscopists could say something was there a little beyond their

ken, and medical men and physiologists could carry it a little further, and take some of the supposed infective germs, and produce a physiological action upon the blood of an animal, and in that way confirm the suspicion that there was something wrong with the water. As to the particular method to be pursued and carried out in researches of the kind, he was pleased to find the Local Government Board bringing its authority to the elucidation of this point. An independent body was taking steps that would tend to set the vexed question of contagion at rest. A very competent gentleman was proceeding to make a series of experiments to ascertain what amount of significance could be attached to current methods of chemical analysis of potable waters. He took samples of water, purposely polluted them with stools of typhoid or enteric fever patients, and compelled animals to partake of them. The results already obtained were startling, and sufficient to confound some who were strong in their belief of chemical analyses, and of those who persisted in jumbling together the evidence of organic impurity and the evidence of unwholesomeness. In the first part of the paper, various ways had been mentioned in which water became contaminated. He desired to point out the great necessity for using precise terms in reference to such matters. Dr. Thudichum had spoken of spring-water. Spring-water was water that many persons would not like to drink. He supposed Dr. Thudichum meant water drawn from subterranean sources at great depths by an artesian well. If this were so, he might be permitted to refer to the inquiry into the Molesey irrigation scheme. It would be remembered that the Molesey people wanted to irrigate certain lands with sewage, and it was discovered that the Lambeth Company was drawing 2,000,000 gallons of its water daily from a gravel-bed subsoil source at Molesey. This underground water was discovered when putting down conduits. The pipes were found to be passing through an immense body of water, and the engineer thought he could not do better than pump it up and use it, and call it spring-water. This was done for a considerable period, and it was supposed the Company were pumping deep well-water. The water was submitted to chemical analysis, and pronounced "perfectly pure and wholesome;" on closer investigation, it was found that the water was in a very bad and unwholesome state. In the course of the judicial inquiry Mr. Michael said: "This is neither more nor less than diluted sewage of a most dangerous nature?" The engineer replied, "Oh no, it is not, for it has been filtered and submitted to our chemist, who pronounces it pure and wholesome water." Among the chemists who pronounced it to be pure and wholesome was, he thought, Dr. Tidy. It had apparently not entered into the calculation of any one, that in drawing subsoil water from an area of some extent (in this instance a radius of more than $1\frac{1}{2}$ mile) the whole incidence of that area must be taken into account. Now, it so happened that at West Molesey it included seven hundred and seventy cesspools, all of which were being pumped dry, and mixed in with the Company's water. A Government investigation ended in putting a stop to that objectionable mode of drawing a supply of "spring-water."

Dr. Tidy said it was a mistake to suppose he had certified to the wholesomeness of this water, on the contrary, he had condemned it.

Mr. Jabez Hogg said he was glad to hear the statement of Dr. Tidy, but he knew that the chemists of the company had expressed an opinion that the water was perfectly pure and wholesome. He could not for a moment doubt Dr. Tidy's word, but there were one or two points in connection with other of his statements which he desired to notice. He had contended that if the Thames River water had a run of a certain number of miles it would tend rapidly to oxidize all the sewage mixed with it. "His results," he said, "were in accordance with those of all the chemists who had examined and reported on the subject; and he also believed that the Thames in its flow of 130 miles as a definite stream did not acquire any increased proportion of organic matter." If Dr. Tidy had examined the water at Lechlade as well as 130 miles lower down, but of which he afforded no evidence, his remarks were apt to mislead. From the first part of his statement it

would appear that the Thames was as pure at Hampton as at Lechlade, the water not having acquired any increased proportion of organic matter; but the results he had published did not show the condition of the water in the river 130 miles below Lechlade; they merely showed its condition after it had passed through the company's filters. Looking, however, solely to the condition of the water after it had been filtered, and applying Dr. Tidy's own theories concerning the rapid destruction of organic matter, and which at Lechlade proceeded from a scantily populated district, and might be taken to be comparatively free from sewage, all organic matter would, according to his theory, have been destroyed long before it reached Hampton; whereas that which replaced it, must contain sewage contamination from numerous populous towns from Lechlade downwards. The organic matter, therefore, even if not large in amount, would be worse in quality, and the water, of course, inferior. In fact, all the towns situated on the banks of the Thames were constantly pouring in large quantities of sewage, and there could be no run of more than 100 yards, to say nothing of 130 miles, where pollution was not going on day and night. Who then could undertake to say when and where some typhoid or malignant fever patient would not be sending excreta into the Thames in a course of 130 miles? Turn to the report of a chemist who differed from Dr. Tidy—the official water-analyst of the Government, Dr. Frankland, whose experience in such matters was beyond all question. He had spoken in his report of the improved condition of London water, which he said was due to the weather and to efficient filtration; but Dr. Frankland's opinions were still strongly adverse to the use of Thames water for drinking purposes, on the ground that it would not be safe so long as sewage found access to it. Actual danger might arise in the production of diseases believed to be propagated by organisms possessing a remarkable degree of vitality; and when seasons conducive to an expidemic outbreak supervened, it was imperatively necessary that water-pipes should not become vehicles for the spread of disease. The important point of divergence between Dr. Frankland and Dr. Tidy, who were both working from the same data, consisted, not in any marked difference as to facts, but in a difference of opinion as to the import of those facts. That was a point which should be clearly understood and weighed when misleading chemical reports were issued to the public. Dr. Tidy of course fell back upon the Registrar General's Reports, as showing that there was no increase of deaths in London; but he omitted altogether to take into consideration how much London had advanced in its sanitation during the last twenty years; how much care had been bestowed by Officers of Health, not only in benefiting the poorer portions of London, by turning out the poor people and letting in light and air, but also in improving the health of London generally. There was scarcely a person, whatever might be his position in life, who had not benefited by what had been effected in that respect. He agreed with the author in his general conclusions, and was ready to admit that he had done a great service in opening out so important a question.

Mr. W. Atkinson said it appeared to him that the whole force of the paper depended upon the question whether zymotic diseases were the result of the growth of living germs in the human frame. The author admitted that water, if it contained dead organic matter, in passing down a stream was purified, and he assumed, what Mr. Atkinson believed had never been proved, that zymotic diseases were dependent upon living organisms of such great vitality that they were almost indestructible. He knew that Professor Tyndall and Mr. Hogg were high authorities on the subject, but he did not know that there was anything to contradict the statement of Dr. Tidy that there was as yet no absolute evidence of living germs propagating those specific diseases. The question of chemical analysis, he thought, had been pretty well cleared up. The author had stated that although chemical analyses did demonstrate the presence of organic impurity, yet it did not enable a decision to be made as to whether it rendered the water unwholesome. That had been fully borne out in a little work by Mr. W. Noel Hartley, Demonstrator of Chemistry at King's College, who stated at

page 23: "Even in very unwholesome waters the amounts of organic matter are exceedingly small. The chemist can tell how much carbon and how much nitrogen this organic matter consists of, but he is powerless to say, by applying any distinctive test, that he is acquainted with the nature of the organic matter, and that it is such as will act as fever poison or as cholera poison."

Mr. CHARLES EKIN said that, at a recent discussion at the Chemical Society on that question, Professor Huxley pronounced an emphatic opinion that water might be as pure as possible from a chemist's point of view, and yet be most deadly; but he did not undertake to say as a physiologist that it was possible to detect the organisms or organic matter contained in it. Mr. Ekin quite agreed with the author and Dr. Thudichum as to the little value to be attached to the determination of organic matter in water, because he had, over and over again, examined water that had undoubtedly given rise to typhoid fever, and found that it contained a very small amount of organic matter, and he had gone into districts where there could be no sort of contamination, and examined the springs, rivers, and brooks, in which he had frequently found large amounts of organic matter, that by no test could be distinguished from the organic matter in sewage. It was well to keep in view the fact that contamination was simply a question of degree. Dr. Thudichum would always go to springs, but he hardly realized the difficulty of getting pure spring-water and keeping it pure. Towns that were using springs for their supply were getting more and more alive to the necessity of buying land around the springs, to prevent the water from being contaminated by high y-manured fields or market gardens. Nearly all the water used for drinking purposes in England must be more or less contaminated, because it was collected on surfaces highly cultivated and thickly populated. With regard to the question of previous sewage contamination, the author overstated the case when he said is was impossible to tell whether the nitric acid and ammonia present in any water had been derived from rain-water or from the soil through which the water had percolated. As a matter of fact it was easy to distinguish between the two, as the amount in rain-water did not exceed a certain very small percentage, and deducting this, the quantity derived from the soil was arrived at. Although the term "previous sewage contamination" was in some respects a misleading one, still there could be no doubt that the determination of the items included under this head afforded useful data in judging of the wholesomeness of drinking water.

Mr. FOLKARD in reply said, on the two questions of the insufficiency of the present methods of chemical analysis, and the danger of using water which had been once polluted, he proposed making a few remarks. With regard to water analysis, the statement which provoked so much controversy, that chemists were powerless to discriminate between wholesome and unwholesome water, he would quote from Memorandum No. 3, on Drinking Water, issued by the Rivers Pollution Commission:—"The existence of an infectious property in water cannot be proved by chemical analysis." If chemists could not tell whether a given water was possessed of infectious power or not, he thought it was fair to say they could not tell whether it was wholesome or not, and therefore the statement in the paper was corroborated by the opinion of Dr. Frankland. Again, he agreed with the opinion frequently expressed by engineers, that a chemist should be able to give a decisive report on a sample from the results of his analysis alone, irrespective of the origin of the sample. If a mineral was submitted for analysis, the chemist or assayer was indifferent as to where it came from or what depth it was obtained. He could report with certainty on the percentage of iron or copper, as the case might be, and if the processes of water analysis were reliable like those of inorganic analysis, water analysts could report with equal certainty whether a given sample was wholesome or not from the results obtained, irrespective of its locality or source. Whether water analysts were willing to give a report when thus left in the dark he left to engineers to decide. He knew that in at least one case this was not so, and that gentleman had had considerable experience, as he had it on good authority that several thousands of samples had passed through his hands. This seemed

to show that neither Dr. Frankland, nor any other experienced water analysts, placed absolute reliance on the results of chemical analysis to show whether a water was wholesome or not, and consequently they agreed so far with the opinion expressed in the paper. It was contended that the great question was, "What is the condition of the water now? not what was its condition fifty years ago, or 50 miles up-stream." This was perfectly true, but unfortunately it was a question which no water analyst could answer. The various processes of water analysis had one and all been shown on chemical grounds to be worthless, and he had endeavored to prove that they were worthless (as far as the power of indicating wholesomeness was concerned) by reasoning which required no technical knowledge to follow it, but simply the exercise of common sense. Eminent water analysts had brought forward apparently conclusive evidence of the worthlessness of all processes of water analysis except their own, and he was convinced that each one of those chemists was right, and begged to refer to their communications on the subject for proofs of worthlessness on chemical grounds. Further, he believed that the cause of the want of confidence of engineers in the results of water analysis was due to the unavoidable employment of defective processes, in the absence of better and reliable ones. That this want of confidence existed he knew, because many of his friends were engineers connected with water-supply, and he ventured to think many could from their own experience corroborate the views at which he had arrived on theoretical grounds. If this were so, the sooner analysts owned it the better, instead of attempting to throw dust in people's eyes, and to bolster up defective methods by saying they had employed them so many thousand times. Consider the method of ascertaining the present condition of a sample of water by the permanganate of potash process. A measured quantity of water was put in a glass standing on a sheet of white paper, and it was noted how many drops of permanganate of potash were required to communicate a permanent pink color to the water. To give it its due, the process certainly had the advantage of simplicity, and after performing the experiment some three hundred or four hundred times it might be a matter of question whether further repetition would greatly add to the operator's skill in water analysis. The sooner the water became pink, the less the amount of foreign matters present; but as to the nature of these substances every one was in the dark, and when it was inquired if Dr. Letheby, who invented the process, or Dr. Tidy, who used it, had established any definite relation between wholesomeness and permanganate, there was no answer. An intelligent lad could master the details of the process in half an hour, while, as before mentioned, the value of the result was admitted by nine-tenths of the analysts of the present day to be *nil*. He thanked Mr. Ekin for supplying an omission in the paper at page 6, line 15. After the words "by the rain in falling" it should have been mentioned that the amount of nitrogen existing as ammonia and nitric acid in rain being very small, anything in excess of the normal amount might, as stated by Mr. Ekin, be fairly put down to animal or vegetable contamination. He could not agree with Mr. Homersham's remarks on hard water. The quantities were so small that it could make but little difference for dietetic purposes whether there were 5 grains or 40 grains of chalk per gallon. Besides many medical men were of opinion that lime in drinking water was essential to the health, at all events, of children, and therefore he could not but think it unfortunate that Dr. Frankland should return such harmless inorganic substances as chalk under the heading of impurities. Although perfectly correct from the chemist's point of view, it was liable to mislead the non-scientific portion of the community. The second question was as to the purification of rivers by natural means. Of course a great deal took place in this way, otherwise (as had been remarked) no one would be alive. Vegetation had a most beneficial influence, although he ventured to think that in nine months of the year in this dull climate the effects could not be very energetic. It must also be remembered that vegetation was supported by inorganic materials, and that the organic matters contained in sewage must decay and be resolved into the salts of ammonia,

carbonic and nitric acids, before they become available for the support of plant life. All this of course took time. The statement made by Dr. Tidy, however, was so extraordinary that it would well repay a little attention. It was to the effect that 10-miles flow was enough for purification (whatever that might mean). The velocity of the river might be assumed to be 2½ miles per hour, whence it followed, according to this theory, that in four hours purification had taken place. If Dr. Tidy meant that river beds showed no signs of sewage 10 miles below the outfall, the statement was probably true, but even that would depend on the ratio of the volume of sewage to the total flow of the river. But the assertion that sewage was decomposed in four or six hours was rather startling. Even admitting this would be the case in the height of summer, during sunshine, and when vegetation was most active (and very few if any chemical actions, especially in dilute solutions, were complete in such a short time), what should be said about the winter months when sunshine was almost an event, and the temperature of the water was near the freezing point, the processes of vegetation and fermentation being nearly suspended? To say nothing of the fifteen hours' darkness of the winter night during which no purification by the aid of vegetation went on (light being essential), and in which time the sewage would flow with the stream 30, 40, or 50 miles. He submitted that the 10-mile estimate was far wilder and more fanciful than any assertions in the paper, in addition to which it was entirely at variance with facts. The Rivers Pollution Commission Report contained two analyses of the water of the Thames, viz., at Reading and at Shiplake paper-mill, and the result showed that after a flow of 4 miles the organic carbon in the water was only reduced to about 6 per cent.; and even assuming that the diminution went on in the same ratio, a flow of at least 64 miles would be required in summer to effect decomposition, the date of the experiment being May 31st, 1873. As a matter of fact, however, such processes were almost invariably more and more sluggish towards the close, in addition to which there was absolutely no evidence to show that the morbific matters (he was half afraid to call them germs) were acted upon in the slightest degree. The above experiments should be pretty conclusive to Dr. Tidy, because the organic carbon was the constituent which agreed so very closely with some of his numerous determinations, and the correspondence of which with his own method he put forward as almost conclusive evidence of the reliability of both processes. After the severe remarks about germs, it was a comfort to him to reflect that he was not the only person who believed in their existence. To his mind the evidence was as conclusive as of the presence of calcium, sodium, iron, &c., in the sun's atmosphere, and in both cases amounted to far more than a probability. To some minds, however, the fact of their not having been seen was to to the possibility of their existence, but it should at least be recognized that several eminent men believed in them. The town referred to in the paper in which an outbreak of enteric fever occurred about three years ago was Caterham. Dr. Thorne Thorne investigated the matter, and made a full report on the subject. The evidence was direct and conclusive that water contaminated with the dejecta of a workman suffering from enteric fever was the cause. An epidemic of typhoid occurred in the village of Lausen, near Basle, Switzerland. The case was investigated by Dr. Hägler, and experiments were made similar to those mentioned by Mr. Baldwin Latham, viz., by throwing about a ton of salt into the water of the stream opposite the cottage in which the first attack of typhoid occurred. In two or three hours' time the water at the village became perceptibly salt, and this was corroborated by the proper test. Some 20 to 30 cwt. of flour were then thrown into the brook, to ascertain if the water was subjected to any filtering process. None of the flour (although well mixed up with the water) arrived at Lausen, conclusively proving that filtration, which was effective in stopping such comparatively coarse particles as those of flour, allowed the specific poison of typhoid to pass in sufficient quantity to strike down 17 per cent. of the population with the disease. A more detailed description had been given in the Proceedings of the Chemical Society, February 17th, 1876. It had

been urged that the outbreak of fever at Caterham would not have occurred if the contaminated water had flowed in contact with the air as a river or brook instead of in closed pipes. Of course this was possible, but it was a mere assumption, unsupported by evidence; fortunately for sanitarians and the public the Lausen case just described set the matter at rest, a mountain stream then being the vehicle of the typhoid poison. After this it would hardly be advisable to rely on germs being destroyed in flowing water. With reference to Mr. Baldwin Latham's remarks on the death-rate of London having slightly decreased, while the impurities in the river water had increased in quantity, it must be remembered that the sewerage system and the sanitary condition of the houses had undergone vast improvements, and therefore to his mind it was exceedingly disappointing that a far greater diminution in the death-rate had not been observed. The late Dr. Letheby pointed out that the real death-rate of London was probably very different from that shown by the Registrar General, the population being continually recruited by young people from the country; also the sick were, in as many cases as possible, removed into the country, and of course many thus died away from home. These causes probably made a difference of at least 5 per 1,000, if not considerably more, and therefore there was no reason to boast of the corrected death-rate of the best sewered city in the world. The statistics of the cholera epidemic of 1854 conlusively showed the ill effects of a foul water-supply, the relative mortalities being as 13 to 4. The fact of the death-rate of the districts of the metropolis, supplied with river water, being the same as that of the Kent Company's district, was doubtless due to the greater number of recruits from the country who settled in the former area. If London were increasing eastward as rapidly as westward the cases would be parallel, and Dr. Tidy's conclusions would hold good, but in view of this great disturbing element (the influx of young people from the country into the western or river-water districts), such comparisons were almost valueless, merely showing that even with such great advantages the river-water area death-rate was not lower than that of the well-water area. He could not admit that the question of storm overflows was irrelevant. It was immaterial to the inhabitants of the lower towns on a river whether these overflows were theoretically necessary or not. The question to them was "did the sewage flow direct to the river in times of heavy rain?" In connection with this subject it should not be forgotten that the sewage thus discharged direct was in its foulest state, the great rush of water flushing the sewers and bringing with it accumulations of filth which had been collecting and festering, possibly for weeks. It would be a question of expense, viz., the construction of sewers in the upper towns large enough to carry off storm water without the necessity of using storm overflows *versus* the obtaining of the water supply of the lower towns from other sources than the river. There could be no doubt that the upper towns would feel it a great hardship to be obliged to spend two or three times as much on their sewerage system from this cause, and in view of the partial and imperfect nature of the remedy this extra outlay would not be justified. He must also dissent from Mr. Latham's inference that low death-rates were the accompaniments of offensive states of rivers. It was probably a mere coincidence and could hardly be taken as proof of the harmlessness of such an abnormal state of things. The fact of malaria usually traveling up stream was irrelevant. It was prevalent in almost uninhabited countries, and was due to conditions of heat and drought simultaneously present in the upper and lower parts of a river. With reference to the effect of water containing the evacuations of cholera patients on the inhabitants of Birmingham, he did not think it was fair to expect an explanation of every case. That injurious effects had followed the use of such water (putting sentiment aside altogether) had been proved in England and on the Continent. It seemed to him that when an admittedly polluted stream was to be used as a source of water-supply the onus of proof of its innocuousness rested on those who proposed it. It was not enough to show that no ill effects had been observed in particular instances. On the contrary, he thought two or three undoubted

cases, of the transmission of disease by such waters, should be enough to condemn them as a class, and prevent wherever possible their use for domestic purposes. Besides, the mere idea was so loathsome that one almost wondered that an attempt should be made to defend it. If "drinking in a circle" were unobjectionable, then why have such refinements as sanitary inspectors, inspectors of nuisances, and food analysts? It certainly seemed inconsistent. The question had been put to him "admitting the presence of germs, was there any evidence to show that they were not amenable to the same laws as organic matter generally?" Here the necessity of extreme precision would be seen. The term organic matter was indefinite. If living organic matter were meant the answer would be self-evident, because germs were living organic matter, and therefore must be amenable to the laws governing such matter. If, on the other hand, his interrogator meant dead organic matter, he replied that germs were no more amenable to the laws of dead organic matter than a living man was. Again, every biologist was aware that the lower the organism the more persistent was its vitality, as a rule, and therefore a living germ was at the very least quite as capable of resisting oxidation during a 10 or 100, or 1,000 miles swim down a river (water being its appropriate medium) as was a hen's egg for an equal time or during transport through an equal distance in its appropriate medium, the atmosphere; and he thought few people would doubt the capacity of a hen's egg to germinate after such an interval and such treatment. Under the circumstances he could leave the members of the Institution to decide which of two chemists was the more likely to gain respect, the one who, after ten years' experience in water analysis, had come to the conclusion that the present methods were unreliable, and was willing to own it; or on the other hand, the one who tried to throw a halo of importance round a process admitted by nine-tenths of the analysts of the present day to be worthless, by stating that he had analyzed nearly four thousand samples by it. It would be equally logical to say that hanging for sheep stealing was a good law because it had (unfortunately) been carried out hundreds of times in this country. In conclusion he must thank the members for the kind way in which they had listened to the paper and to his remarks, and if it should be the means of directing still further attention to this important subject he should be extremely gratified.

CORRESPONDENCE.

Mr. H. Percy Boulnois said that the Water Works of the City of Exeter, of which he had charge, were the property of the Corporation. The daily supply amounting to 1,280,000 gallons, was pumped from the river Exe, the intake being situated about 4 miles above Exeter and 12 miles below the town of Tiverton, the sewage of some ten thousand persons at this place being daily passed direct into the river in a crude state.

To ascertain how far this sewage contamination chemically affected the water, he took samples from different points in the river in August, 1880, and submitted them to Mr. F. P. Perkins, the public analyst of the City of Exeter, who examined them by the permanganate process and a modification of Professor Dittmar's carbon process. The following Table (see next page) embobied the results of these tests.

It would be noted, on reference to this Table, that the water at the intake was chemically nearly similar to that above Tiverton, and that this result was obtained gradually by the water on its journey. The Dart stream, however, seemed to pollute the water, there being a marked difference between samples 4 and 6; this was accounted for by the fact that the Dart rose on Exmoor, and although it could receive absolutely no sewage contamination, it was brown with peat, and this gave a bad analysis.

So far as Exeter was concerned, it was contended that the water at the intake was not unhealthily affected by the sewawe contamination of Tiverton, and this result might be attributed to the following causes: (1) The excessive dilution of the sewage with a large bulk of pure water. (2) The oxidation which the water underwent on its 12 miles journey from Tiverton, tumbling as it did over two weirs and rushing over many a shallow and stony bed. (3) The action upon

SPECIMENS OF WATER TAKEN BY MR. BOULNOIS FROM THE RIVER EXE ON AUGUST 16TH, 1880, AND SUBMITTED TO MR. PERKINS FOR ANALYSIS.

Number of specimen.	Where obtained.	Distance below Tiverton.	Amount of organic impurity in 100,000 parts.	
			Oxygen consumed $\times \frac{c}{o} =$	Organic carbon yielded.
1	Above Tiverton	1 mile above	.0718 × 2.27 =	.163
2	Below Tiverton	100 yards below	.0873 × 2.81 =	.246
3	Ditto	2 miles "	.0929 × 2.93 =	.273
4	Bickleigh Bridge	3 " "	.0788 × 2.41 =	.190
5	In a stream joining the Exe called the Dart	3¼ " "	.2070 × 2.11 =	.436
6	Below Bickleigh mill stream.	3½ " "	.0859 × 3.16 =	.272
7	Bourne Farm	5 " "	.080 × 2.70 =	.218
8	Thornetown above the weir	8 " "	.0831 × 2.60 =	.218
9	At intake	12 " "	.0715 × 2.29 =	.164

the water by aquatic plants and weeds, and of the soil of the river banks and bed. (4) The constant evaporation from the surface of the water, and consequent molecular changes thus altering its character. (5) Other unknown causes possibly at work which made up the ever active processes of Nature's great laboratory.

The author questioned the reliability of chemical analysis to detect "previous sewage contamination," but he did not appear to have given credit to the fact that, in a properly conducted analysis, no chemist relied upon one indication only, but that all the bearings of the analysis and history of the water were considered. If the analysts' evidence was to be doubted, much difficulty would be experienced by sanitary authorities in closing polluted wells or other impure sources of water supply; but hitherto reliance had always been placed upon such evidence, and he thought no sufficient proof had been adduced in the paper to shake public confidence. The question was one of grave importance, the health of a community being no doubt greatly affected by the character of its water supply; no hasty conclusion should therefore be arrived at in favor of deep well water. It might be that the terrible "diseases of the stomach and intestines" mentioned in the paper were due to contaminations in shallow well waters, or to the mineral substances found in most deep well waters, and not from that source which Nature pointed out as the most convenient and proper from which to derive the water supply.

Mr. EDWIN CHADWICK, C.B., observed that there were particles from small-pox and other eruptive diseases, which were known to be distributed in hospitals within measurable distances. But these were imagined, but not proved, to be germs of specific diseases which spread to immeasurable distances, and which it was averred must be productive of the same diseases. These germs were alleged to be the cause of enteric fever, and when conveyed by water carriage must generate it. A disease did arise sometimes, with varying type, from the emanations from stagnant drains or sewers. But he never heard of any arising in such conditions along lines of sewer in accordance with the germ theory. In an address given at Croydon to the members of the International Medical Congress by Dr. Alfred Carpenter, adducing experiences in answer to the violent objections that had been made by the advocates of chemical disinfectants, and other processes against sewage farms, on the grounds that they must receive and must spread the germs of infectious disease. Dr. Carpenter stated the result of his experience, to which he would direct particular attention: it was as follows:

"The non-infectious character of the excretions of those suffering from epidemic and infectious diseases when distributed upon a sewage farm is proved by the fact that there have been occasional outbreaks of infectious diseases

in Croydon during the past ten years, including two epidemics of small-pox, several outbreaks of scarlet fever, occasional cases of diphtheria, and three periods of typhoid prevalence—two of which were distinctly connected with contamination of water supply in its distribution, and a third was distributed by means of milk. In the years 1875–76 the excreta of at least a thousand cases of enteric fever were utilized on the farm. In the majority of the cases the excreta were certainly not disinfected, and had they been capable of setting up the disease, some of the sixty-five persons at that time in the employ of the Local Board must have suffered from the infection. Cases which did arise were not on the farm, or even in the majority of cases, near to it; they were on the hills, beyond the range even of subsoil water. The changes in sewage are not in any way similar to those which have been known to take place in poudrette and other particular forms of dried ordure. There is no doubt in my mind of the destruction upon sewage farms of the germs of mischief, which, when unaltered, may be capable of setting up zymotic disease. They are not preserved as they may be in dried ordure, or in other products in which so-called disinfectants have been used, which have simply preserved the germs from decay; but they are chemically and physically altered so that mischief cannot arise. This result has been also found to apply to the excreta of animals suffering from epizotic disease. During the past few years there have been several outbreaks of infectious pleuro-pneumonia in the Croydon district, the infection being brought from the Metropolitan Meat Markets. The cow-sheds in which the disease has arisen have drained into the Croydon sewers, and blood and excreta from the slaughtered animals have been washed down those sewers. The sewers have carried the morbid matter from the sheds to the farm; but there has been no corresponding disease among the cattle upon the farm."

To this he might add that similar demonstrations were presented by all well worked sewage farms. Moreover, insects generated and distributed in solid manures, and in stagnant semi-liquefied manures, were drowned by liquid manures in active circulation. It must follow that from continued exposure to such germs as those assumed that the health of those working on the sewage farms must be lower than the average, whereas it has been shown in a report to the Royal Agricultural Society that the health of the people working and living on the sewage farms was remarkably higher than the average.

Mr. C. E. De Rance remarked that the author, by grouping a series of well-known facts in a definite connection, had done useful work, in establishing the unassailable result, that the practical freedom of drinking water from organic impurity must be absolute to prevent the spread of zymotic disease. How this desirable condition was to be obtained was a difficult problem. Gravitation supplies, derived even from the mountain slopes of Wales and the English Lake District, traversed only by mountain sheep, occasional tourists, shepherds and their dogs, were liable to receive the germs of entozoa, especially from the latter; while water supplies abstracted from rivers, even when all town sewerage was intercepted, received streams flowing past polluted farm yards, and the soakage from the offensive ditches with choked outlets, which so often surrounded them. In a gravitation supply absolute freedom must of necessity be impossible, but much could be effected, by making the separation of sewerage and storm water compulsory, not only in the drainage from cities and towns, but in the effluent water from country estates.

In water obtained from underground sources, whether from deep-seated springs, or wells, the chances of poisonous germs being left was very small, after the passage of the water through several hundred feet of porous rocks, provided that the water had passed through the texture of the rock, but in many cases, the water had simply traveled, both vertically and horizontally, through open fissures formed by joints and faults, and this was probably the condition of many wells giving an exceptionally large daily yield of water, which had not been naturally filtered. In some other cases, deep bore holes had been sunk entirely in porous rock, in which every care was taken to exclude, and tube out, surface waters, but the water yielded was found to be polluted, percolation having taken place through cracks and fissures, connecting the surface with the saturated portion of the rock beneath. Of necessity wells reaching porous formations after passing through a zone of impermeable material were not open to this objection, and the chances of pollution were exceedingly small in the water yielded by them and by deep-seated springs. To increase the yield of these springs appeared to be a matter of the highest importance, for

should the construction of "dumb wells" become general, and the drainage of impermeable lands be artificially carried to porous strata beneath, whenever practicable, the supply of pure drinking water would not only be increased, but the absorption of excessive rainfalls would diminish the intensity of floods, and improve the dry-weather volume of the streams.

Mr. H. U. McKie knew one town in Wales which took its water supply from a river, when about one mile of extra piping would have given good spring water. Villagers near the river from which the water was taken would not use it, yet chemists pronounced it pure. He had recently had occasion to examine some works by a river side, and saw what he thought to be two sticks floating down the rippled surface of the stream; they appeared to be attached together by a string, and made curious bobbing motions, similar to a float on a fishing-rod when there was a nibble at the bait; on closer examination he found it was a large salmon so covered with a fungoid growth as to be both pitiable and revolting, and he was told that the river was full of salmon thus affected. Now, as this disease also attacked trout, ells, and other fish, in the river, he thought it right to ask if water so contaminated could be a safe source of potable water supply for a town? He knew of two towns on this river which derived their water supply from it, and there might be others.

Mr. H. Robinson could not agree with the author in his sweeping condemnation of the use of river water unless taken near the source. However desirable it might be to obtain water free from the risk of contamination (and every engineer aimed at securing such a supply) in practice it would be impossible to meet the wants of the community; if the rule laid down were acted on. The enforcement of this rule would necessitate the abandonment of numerous sources of supply which failed to comply with these conditions, but which, although subject to the risks referred to, had not produced any evil results. Probably the author, by enforcing an unreasonably high standard of purity, would create some of the evils which it was sought to prevent. If only water from deep subterranean sources or from streams above suspicion of contamination were to be used, a less abundant supply would be available than was now employed. The limitation of supply would arise from two causes, one being the difficulty of obtaining the necessary quantity of underground water, and the other being the cost of getting it. Where the cost of supplying a town was attended with heavy water rates, Mr. Robinson had found that the authorities were disposed to restrict the quantity used for sanitary purposes, such as flushing sewers, road watering, and the like. Such restriction would lead to insanitary results. The alarmist views entertained by the author were not supported by practical evidence. If the germs of contagious diseases had the vitality and produced the mischief alleged, the evils attending the use of water subject to their influence would have been manifested. Without wishing to underestimate the risk of transmitting diseases by water, Mr. Robinson would expect to find some proof of the allegation in the case of a city like London. Obviously the water supplied by the metropolitan companies which took their supply from the Thames must be placed in the class of water of the dangerous kind; no contagious diseases, however, could be traced to its use. Frequent attempts had been made to connect cases of typhoid and similar diseases to the use of water supplied from the Thames, and he had on several occasions been engaged in examining into such cases. He had found (and the experience of others was to the same effect) that where water had caused illness it had been solely through the foul state of the cisterns and receptacles for storing it. The presence of filth of various kinds and dead animals accounted for the mischief. A constant supply would remove this cause of danger.

Another view of the subject was worth referring to. Supposing water perfectly free from suspicion was to be insisted on for dietetic purposes, a duplicate supply would be required in many cases, such as has been proposed for London. Were this system to be adopted the inferior water would most probably be less pure than that previously supplied, inasmuch as it would be thought unneces-

sary to filter water intended to extinguish fires, water streets, or cleanse courts, and alleys. The germs of some contagious diseases were, according to the best medical authorities, even more capable of being introduced into the human system through the lungs than through the stomach. If, therefore, the dangers apprehended were really based upon reasonable grounds, the air instead of the water might become the medium for conveying the disease germs under the state of things that would then exist. Much inconvenience had been experienced by engineers, owing to analytical chemists adopting different terms to express the results of their analyses. Mr. Robinson was continually having to deal with analyses in which similar impurities were described by different chemists in different terms. The adoption of a uniform nomenclature would be both convenient to those who had to act upon the results of chemical analyses, and would also remove one of the several grounds of difference that appeared to exist amongst chemists themselves.

SOME EXPERIMENTS IN THE TRANSMISSION OF POWER BY ELECTRICITY.*

By GEORGE and WILLIAM E. GIBBS.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

DESCRIPTION OF GENERATOR AND MOTOR.

The dynamo-electric machine used as a generator was one of Mr. Weston's latest pattern, known as the "fifty light incandescent machine." The machine used as a motor was identical with the preceding, except that it was only intended to run forty lights.

The machines were of the derived field type, that is, the field magnets were wound with comparatively fine wire, so that their resistance was about 800 times the resistance of the armature. The terminals of the field wire were connected with the brushes directly, and there fore when the machine was running the magnets became charged even if the main circuit was not closed.

In this machine the magnets are horizontally arranged above and below the armature. They are essentially two horse-shoe magnets with like poles turned toward each other. The armature is wound with a continuous heavy wire which is brought out at every turn into a loop and soldered to the commutator.

The core of the armature is made up of thin wrought iron discs separated by small washers of gelatinized fiber. The discs are shaped somewhat like a gear wheel, that is, they have teeth on the edge to the number of perhaps twenty and of the width of one-fourth of an inch, so that when the armature is complete there are a number of ridges running its whole length parallel to the axis. In the hollows between the ridges is wound the wire of the armature in a single layer, which, when finished, is of the same height as the ridges, making the whole a true cylinder. The ridges are called "polar extensions," for by projecting through the layer of wire they come very near to the field magnets and increase the polarity of the armature when the machine is running, and consequently the intensity of the lines of magnetic force. The core is, moreover, pierced from end to end with several holes arranged at equal angular distances apart, and the discs of which it is made up being separated from each other by the space of about a twentieth of an inch, a complete system of ventilation is kept up by the action of the machine, and the armature is thus kept cool. Each disc has also two radial slots cut in it to prevent the formation of an extra currrent. The commutator is composed of copper sectors separated by gelatinized fiber strips.

The brushes are of silver plated copper, each brush being composed of several strips, placed on one another, so that although the brush has great flexibility it has also sufficient springi-

* Abstract of a Thesis written at the Stevens Institute of Technology.

ness to cause it to press uniformly on the commutator.

Each brush is, besides, held in a spring clamp, which yields to any inequality of the commutator. The brushes are adjustable at any angle about their axis, and are in practice turned to the point of least sparking, which is the neutral plane of the machine. When properly adjusted the sparking is inappreciable.

The wire from each pair of field magnets terminates at a binding post on the top of the machine. When the generator is working to its full capacity these posts are connected by a short wire, but when it is desirable to use only part of the power of the machine, a variable resistance is placed between them. By altering this resistance the intensity of the magnetic field is varied, and the work done may be perfectly controlled. The resistance of the armature was .03 ohms, and the resistance of the field was 24.5 ohms, measured while warm, immediately after the experiments ceased.

DESCRIPTION OF DYNAMOMETER.

In measuring the power transmitted from the engine to the generator, the Kent dynamometer built by the class of '76 of the Stevens Institute was used. In this dynamometer the receiving and transmitting pulleys are each carried by a separate shaft. These shafts are in the same straight line, and upon the ends which face each other there are two bevel wheels. A third bevel wheel at right angles to these two connects them and transmits motion from one shaft to the other. This wheel is loose upon its axis, which is prolonged to form a pendulum, and is supported by a brass pin passing through it and fitting into holes in the transmitting and receiving shafts. A heavy weight is attached to the end of the pendulum, and when the machine is running the pendulum is deflected from the normal vertical position to a position approaching more or less the horizontal.

The sine of the angle of deflection, the weight of the pendulum and "bob," and the number of revolutions per minute determine the power transmitted.

The dynamometer was standardized as follows:

The pendulum was supported in a horizontal position by a prop at a distance of two feet from the center of the pin connecting the shafts. The lower end of the prop rested on a platform scale.

The weight indicated was 170.5 lbs., and since the lever arm of this weight is divided by two, by the arrangement of gear wheels above described, the weight at *one* foot is 170.5 pounds.

Then to get the power transmitted we have,

$W = 170.5 \times \text{sine}\ \theta \times (6.28 = 2\pi) \times$ number of revolutions.

Where W = work done in ft. lbs. per min.

Of this power, however, a certain percentage is lost in overcoming the friction of the bearings and must be allowed for.

To find the friction, the main and field circuits of the generator were broken but the brushes left on the machine. A seven-inch pulley was fastened on the shaft of the generator, close to the twelve-inch driving pulley. On the small pulley a prony brake was arranged, so that when the engine was transmitting power to the generator through the dynamometer the energy absorbed by the brake was substituted directly for the electrical energy developed by the machine when the circuits were closed.

Several experiments were made at different deflections of the pendulum.

The variation was not great, but the mean is given.

The speed was constant, and was the same as in all the experiments on the efficiency of generator.

Dynamometer:

Sine of deflection=.33.
Weight=170.5 lbs.
Radius of driving pulley=16 inches.
Therefore the constant pressure indicated $= \frac{170.5 \times .33}{1.33} = 42.3$ lbs.

Prony brake:

Length of arm=30 inches.
Pressure on scale=7.25 lbs.
Pressure at circumference of pulley $= 7.25 \times \frac{30}{6} = 36.25$ lbs.

Since the dynamo pulley is 12 in diameter; $42.30 - 36.25 = 6.05$ = loss by friction, and $\frac{6.05}{42.3} = 14.2$ per cent. friction.

FRICTION OF ARMATURE BEARINGS.

Lack of suitable apparatus prevented us from determining this experimentally, but since it has been found for similar machines by repeated experiment to be less than 3 per cent., and since the bearings in the machine used were as nearly perfect as skilful workmen and accurate mechanical means could make them (being a steel shaft running in gun metal bearings), we felt at liberty to assume the friction as 2.5 per cent. The bearings were oiled by continuous oilers and the heating was so small as to be imperceptible even after long runs.

EFFICIENCY OF GENERATOR.

In making tests for the efficiency of the generator, the current generated was carried by iron wire resistances running across the room from side to side in the open air, so that the heat generated was rapidly conducted away.

A switch was so arranged that the generator could be instantly thrown out of circuit and the resistance of the line measured within five seconds. In this way the varying effect of temperature on the resistance was eliminated.

All resistances were measured by a Thomson high resistance galvanometer. An electric lamp placed in a magic lantern threw a ray of light on the galvanometer mirror, which was reflected to a screen. This gave an immensely magnified motion to the image so that the scale could be read from some distance in a well-lighted room.

CALORIMETER TEST.

In determining the electrical energy developed by this method, a calorimeter was used in circuit with the iron wire resistance.

This calorimeter consisted of a cylindrical vessel of galvanized iron imbedded in sawdust in a wooden box. By this means any great waste of heat by conduction and radiation was prevented; but as some heat must have been conducted by the wood, it was allowed for in each case by taking water at the atmospheric temperature and cooling it by means of ice to as many degrees below that temperature as it was to be raised above it by the heating of the coil. In this way the transfer of heat from the sawdust to the water during the first half of the experiment was equal to the transfer from the water to the sawdust during the second half. The electrical energy expended in the calorimeter was measured by its heating effect on a coil of German-silver wire. The wire used in the coil was of No. 8. B. W. G.

The coil itself was entirely immersed in the water, and its ends were soldered to two copper rods which were fastened in the calorimeter cover. In this way the high resistance wire being entirely under water, any over-heating was prevented. The resistance of the coil was exactly .09 ohm at 74° Fahr. in the water.

Distilled water was used in the calorimeter, it having a much higher resistance than ordinary water, thus diminishing the tendency of the current to pass through the water from one turn of the coil to another. No evidence of such an action having taken place was, however, observed at the conclusion of the tests.

An uniform temperature of the water in the calorimeter was secured by using two miniature screw-propellers of wood which were constantly turned in the water during the experiment.

When everything was ready for the test the generator was run until the circuit was thoroughly heated, and its resistance remained constant.

The calorimeter was then thrown into the circuit and an equal resistance of circuit thrown out, so as not to alter the total resistance. At the end of the test the resistance was measured as soon as the circuit was broken and before the wires had cooled.

DATA FROM THIS TEST.

Weight of calorimeter empty, 31 pounds.
Weight of calorimeter full, 58.25 pounds.
Weight of water in calorimeter, 27.25 pounds.
Range of temperature, $=91.°2-68.°6=22.°6$ Fahr.
Specific heat for above range, $=1.018$.
Time of test, $=25$ minutes.
Resistance of iron wires and calorimeter coil, $=.484$ ohm.
This resistance and field in multiple are $=.475$ ohm.
Total resistance of circuit, $=.475+.03=.505$.
Resistance of calorimeter coil, $=.09$ ohm.
Ratio of resistance of total circuit to resistance of calorimeter coil, $=\frac{.505}{.09}=5.61$.

RESULTS.

Energy developed in calorimeter = $\frac{27.21\times1.018\times22°.6\times772}{25}=19330.89$ ft. lbs. per minute.

Total electrical energy developed in circuit, $19330.89\times5.61=108446.28$ ft. lbs. per minute.

Determination of the energy transmitted by the dynamometer in this test:

Speed of dynamometer, =340 revs. per min.

Sine of the angle of deflection, =.36.

Therefore, indicated energy = $170.5\times.36\times6.28\times340=131056.4$ ft. lbs. per min.

Determination of the efficiency of generator from the above:

Energy consumed in turning armature in field of force = $131056.4\times.858=112446.$ ft. lbs. per min.

$\therefore$ Efficiency $=\frac{108446.28}{112446}=.963.$

That is, 96.3 per cent. of the power applied to the armature pulley appears as electrical energy in circuit and magnet coils.

Now, to find the "commercial efficiency," or the ratio of the mechanical energy required to drive the dynamo (including friction of armature bearings and agitation of air by armature) to the electrical energy which appears in external circuit, we have:

Energy actually applied to armature pulley=total indicated energy less the friction of the dynamometer=$131056.4\times.883=115722.8$ ft. lbs. per min. Of the total electrical energy generated there appeared in the armature—$108446.28\times\frac{.03}{.505}=6442.35$ ft. lbs.

And the electrical energy consumed in the field circuit, which appeared partly as heat and was partly used in magnetizing the cores=$108446.28\times\frac{505}{24.51}=.02\times108446.28=2168.92$ ft. lbs. per min.

Then the total internal work=$2168.92+6442.35=8611.27$ ft. lbs. per min.

Therefore the amount of energy appearing in external circuit=$108446.28-8611.27=99835.01$ ft. lbs. and the commercial efficiency=$\frac{99835.01}{115722.8}=.866.$

TESTS BY MEASUREMENT OF THE ELECTRO-MOTIVE FORCE AND RESISTANCE.

In order to determine the electrical energy by this method, we first measured the electro-motive force of the machine and the resistance of the line very accurately. From these data we found the current flowing by the formula $c=\frac{E}{R}$. Then, knowing the current, the electrical energy developed in external circuit is given by the following empirical formula—$c^2R\times44.24$=energy in ft. lbs. per min.

The electro-motive force was measured between the binding posts of the generator by means of a condenser and a Thomson high resistance galvanometer.

The standard of electro-motive force employed was the Latimer Clarke cell. Two of these cells were obtained newly made up from the Western Union Telegraph Company. They were allowed to charge a micro-farad condenser, and the condenser was then discharged through the galvanometer. A number of experiments were made with these in order to determine accurately the deflection on the scale corresponding to a cell. This deflection is proportional to the current flowing through the galvanometer coils, and, consequently, of the charge held by the condenser, which depends upon the electro-motive force of the charging cell.

The deflection corresponding to one cell was found to be exactly five divisions of the scale. Elliott Bro.'s switch was used to connect the dynamo and galvanometer alternately with the condenser.

The connections were made as perfect as possible by amalganation.

DATA.

Capacity of condenser=.05 micro-farad. Deflection of galvanometer with condenser charged by cell=5 divisions. Average deflection of galvanometer with condenser charged by dynamo=103.5 divisions. Electro-motive force of cell=1.457 volts.

Therefore, $5 : 1.457 :: 103.5 : (x = 30.159$ volts). Resistance of line while hot=.431 ohm. Since the electro-motive force was measured between the binding-posts, the resistance of the armature was excluded.

Resistance of armature=.03 ohm.

∴ Resistance between binding-posts=.401 ohm.

Then $-c=\frac{E}{R}$

$$c=\frac{30.159}{.401}=75.2 \text{ webers.}$$

Energy developed in external circuit=$c^2R\times44.24=$
$(75:2)^2\times.401\times44.24=100330.8$ ft. lbs. per min.

Total electrical energy—

$$100330.8\times\frac{.431}{.401}=107352.95 \text{ ft. lbs. per min.}$$

Energy indicated by dynamometer—

Sine of mean deflection=.352
Mean speed= 340 revs.

Indicated energy—

$170.5\times.352\times6.28\times340=128306$ ft. lbs. per min.

Applied energy (equal total energy minus combined friction)$=128306\times.868$ $=110087$ ft. lbs.

Therefore, efficiency of machine$=\frac{107352.95}{110087}=.975$ or 97.5 per cent. actually appeared as electrical energy in external and field circuits.

Determination of the commercial eff.

Energy actually applied to armature pulley—

$128306\times.883=113294.19$ ft. lbs. per min.

Of this there appeared in the armature—

$$107352.95\times\frac{.03}{.431}=7472.35 \text{ ft. lbs. per min.}$$

And in the field circuit—

$$107352.95\times\frac{.431}{24.51}=1887.76 \text{ ft. lbs. per min.}$$

Therefore, total internal work =

$7472.35+1887.76=9360.11$ ft. lbs. per min.

Then there appeared in external circuit—

$107352.95-9360.11=97992.84$ ft. lbs.

And commercial efficiency=

$$\frac{97992.84}{113294.19}=.864.$$

The resultant efficiency of the generator will be the mean of the two efficiencies as determined by the two methods, or;

Average efficiency=.969
Average commercial efficiency=.865.

EFFICIENCY OF MOTOR.

In determining the efficiency of the motor as a machine for converting electrical energy into mechanical, we connected the generator and motor by heavy copper rods in order to reduce the loss of energy in the line to a minimum. A prony brake was applied to the pulley of the motor and the pressure of its arm upon a platform scale measured directly. This gave an accurate indication of the power of the motor.

To avoid heating of the brake by friction, it was arranged in such a manner that a stream of cold water entered it at the top, and after passing through it to the pulley, escaped by a hole in the bottom. In this way we were enabled to make runs of any length of time. Between the nuts which tightened the brake, and the brake itself, were placed thick rubber washers, which by their elasticity yielded to any inequality of motion, and kept the speed and corresponding pressure on the scale very constant.

By means of the brake we could apply variable loads and get various ratios between the speeds of the two machines.

The electrical energy entering the motor was controled by altering the variable resistance in the field of the generator.

Although this alteration diminished the intensity of the magnetic field, the work done in the coils did not vary until after the third decimal place, so the commercial efficiency of the machine remained constant.

The conditions, however, having been altered, the results are not such as can be plotted in a curve.

It is to be remarked, that in these experiments, the machines which we used were so large that it was not possible to work them up to their full capacity, the dynamometer being unable to transmit sufficient power.

The results obtained are tabulated as follows:

DYNAMO.

No.	Speed.	Sine of deflection.	Indicated power in ft. lbs.	Per cent. applied to armature.	Actual energy applied to armature.	Com. effi.	Ft. lbs. current in external circuit.
1	405	.355	153945.6	.883	135928	.638	86717
2	405	.420	182132.8	.883	160820	.638	102603
3	405	.475	206983.6	.883	182763	.638	116000
4	405	.515	223329.5	.883	197200	.638	125813
5	405	.565	243060.6	.883	214620	.638	136927
6	405	.520	225497.8	.883	169910	.638	108402
7	405	.585	253685.0	.883	223995	.638	142905
8	405	.66	286208.8	.883	254921	.638	152638
9	405	.31	134431.4	.883	118701	.638	75730
10	405	.345	149609.1	.883	132104	.638	84279
11	405	.350	152777.3	.883	134895	.638	86059
12	405	.360	156113.9	.883	137847	.638	87941
13	405	.515	223329.5	.883	197200	638	125823.

MOTOR.

No.	Speed	Wt.	Ft. lbs. given out by motor.	Ft. lbs. of current in external circuit.	Effi. of mortar.
1	932	2	29264.8	86717.0	.337
2	892	4	56017.6	102603.0	.545
3	860	6	81012.0	116000.8	.699
4	844	8	106006.4	125813.0	.842
5	800	10	125600.0	136927.0	.917
6	1042	4	65437.6	108402.0	.603
7	1021	6	96178.2	142905.0	.671
8	1185	4	74418.0	152638.9	.481
9	763	4	47916.4	75730.6	.633
10	717	6	67541.4	82279.8	.809
11	572	8	71843.2	86059.8	.834
12	564	8	70838.4	87941.9	.806
13	738	10	115866.0	125823.6	.921

No.	Work done by motor.	Work absorbed by generator.	Efficiency of combination.
1	29264.8	135928	.214
2	56017.6	160820	.348
3	81012.0	182763	.443
4	106006.4	197200	.539
5	125600.0	214620	.580
6	65437.6	169910	.390
7	96178.2	223995	.429
8	74418.0	254921	.292
9	47916.4	118701	.403
10	67541.4	132104	.511
11	71843.2	134895	.532
12	70838.4	137847	.515
13	115866.	197200	.589

EFFICIENCY OF MOTOR.

The motor, as a machine for converting electrical energy into mechanical, seems to be excellently adapted to the purpose. The only point that admits of improvement is probably the resistance of the magnet coils, which should be higher in proportion to the resistance of the armature, thus taking less current to keep up the magnetic field.

EFFICIENCY OF THE COMBINATION.

It is when the generator and motor are coupled together that the efficiency of the whole falls, as is shown by the tables, to such a low percentage.

The reason for this, however, and the means of remedying it seem obvious.

By one of the fundamental laws of electricity, we know that the work done in any portion of an electric circuit is directly proportional to its resistance.

In the case of the two machines coupled, as in the above series of experiments, the resistance of each was very low and equal, while the resistance of the line was practically nothing.

Under these conditions, nearly half the work must necessarily be done in the generator, and the results verified this law.

In order then to increase the efficiency of the combination, more work propor-

tionally must be done in the motor and less in the generator. To accomplish this, we find by applying the above rule that one of two things may be done, we may either decrease the resistance of the generator or increase the resistance of the motor. In practice, a compromise would probably be made, that is, the generator armature would have its resistance reduced, and the motor have the resistance of its armature raised sufficiently to cause nearly all the work to be done in the motor.

This applies to a single motor.

Where several motors were supplied with current from a single machine they would probably be arranged in "multiple arc," and be of such a resistance that they would take only a certain amount of current, and, when coupled up with the generator, their resulting resistance would be the same as would be given to a single motor doing the combined work of all.

In this way each machine does in a measure induce its own current and controls the current generated, so that if only one motor is running, the current generated is only sufficient for it and as each one is put in circuit the current increases in a ratio which just keeps each motor supplied with the proper amount of current.

When by a course of experiment the proper ratio of resistances shall have been determined, there seems to be no reason why the combined efficiency should be below eighty per cent.

THE ROLLING STOCK OF THE ST. GOTHARD RAILWAY.

By R. ABT.

From "Organ für die Fortschritte des Eisenbahnwesens," for Transactions of the Institution of Civil Engineers.

ALTHOUGH this railway is to be opened to traffic this year the rolling stock is still wanting, and great discussion has taken place on the question, especially as to whether the engines are to be tank- or tender-engines. Whilst the existing Alpine lines are satisfactorily worked by tender-engines, the frequency of good water stations on the St. Gothard, with other advantages, spoke strongly for the use of tank-engines. To decide this and other questions a careful study has been made of the locomotive working in Switzerland and other countries.

The total length to be worked by the engines of the St. Gothard line, including four branches, may be taken at 291 kilometers (180) miles. It was at first considered that the yearly traffic for the first ten years might be taken at 200,000 passengers and 400,000 tons of goods. Subsequently the estimate has been raised to about 250,000 passengers and 450,000 tons of goods; the traffic being, of course, greater on the main line through the tunnel, and less on the branches.

With regard to the ratio between dead weights and paying weights, it appears that on the Swiss railways the number of seats occupied as compared with the number provided, taking the average from 1874 to 1879, was 30.2 per cent. On the St. Gothard line it was estimated that it would be 40 per cent. Again, the paying load for goods during the same years on the Swiss railways averaged 27.51 per cent. of the gross load. Owing to the heavy traffic of the St. Gothard railway the proportion was estimated at 40 per cent. The dead weight of carriages per seat provided, for four-wheeled American cars, varies from 221 to 305 kilograms. For the carriages of the St. Gothard line it is 266 kilograms for four-wheeled and 186 for eight-wheeled carriages. On the whole a weight of 250 kilograms per seat may be assumed, which is equal to 605 kilograms per passenger, or 700 kilograms for passenger and dead weight together. Again, the average of the Swiss lines for goods wagons is 0.55 ton as the tare per ton gross weight hauled; and since only 40 per cent. of the gross capacity is utilized, the dead weight per ton of paying load is 1.375 ton, giving 2.375 tons as gross weight per ton of paying load. Hence results the following as the estimated traffic on the various divisions of the St. Gothard railway:

Line.	Traffic.	Gross weight hauled per annum. Tons. (The metric ton= 0.9842 av. ton.	Ditto per day. Tons.
Immensee to Bellinzona.....	Passenger.....	19,600	537
	Goods.....	187,500	3,254
Bellinzona to Chiasso.....	Passenger.....	175,000	480
	Goods.....	960,000	2,606
Bellinzona to Pino.....	Passenger.....	175,000	480
	Goods.....	237,000	651
Bellinzona to Locarno.....	Passenger.....	105,000	288
	Goods.....	23,750	65

With regard to speed, the actual speeds on the Mont Cenis (gradient 1 in 33) are:

Express trains.... 15 to 18 miles an hour.
Ordinary " .... 14 to 16 " "
Goods " .... 12 to 14 " "

On the Brenner-Semmering the speeds are:

Passenger trains, average 12 miles an hour.
Goods " " 7 "

Herr Gottschalk holds that a goods engine on such lines, gradient 1 in 40, should never exceed 9 miles an hour. Herr Hellwag fixed the conditions for the St. Gothard railway as follows:

		Miles an hour.
In the valley, max. gradient 1 in 100...	Passenger trains,	27
	Goods "	10
In the mount'ns, max. gradient, 2.7 in 100.	Passenger "	13
	Goods "	7
In the tunnel, max. gradient, 2.58 in 100	Passenger "	18
	Goods "	9

With regard to the number of trains, allowing four hours out of the twenty-four for delays, and that passenger trains are thirty-one minutes, and goods trains sixty-three minutes, between Göschenen and Airolo, the possible number of trains per day would be twenty-five. If a crossing place were provided in the tunnel, the number could be raised to thirty-seven. With regard to the train loads, the terrible effects of a train breaking loose on such a line make it necessary to limit this according to the strength of the couplings. Even with the latest form of couplings it is considered that the total stress should not exceed 6½ tons. On the Semmering, on gradients 1 in 40 and curves of 200 yards radius, this stress is reached with goods trains of 200 tons.

On the St. Gothard railway the gradient is 1 in 37, but the curves have only 300-yards radius. The result will therefore be the same, and the greatest weight of train must therefore be taken as 200 tons.

The locomotives necessary for conveying the traffic under these conditions for the first year were estimated as follows:

12	engines,	4-coupled,	25	tons adhesion wt.	
19	"	6 "	38	"	"
17	"	8 "	52	"	"
—			—	"	"
Total 48	"		1,906	"	"

For subsequent years the number was taken at eighty. The railway already possesses fourteen engines, and thirty-four new ones will therefore be required when the line is opened. In October, 1880, the directors contracted for the supply of thirty-seven engines as follows:

Six tank-engines, four-coupled, with a four-wheeled bogie, for the passenger trains on the valley sections: diameter of cylinder, 16½ inches; stroke, 24 inches; total heating surface, 1,120 square feet; weight loaded, 42.7 tons; smallest adhesion weight, 22.5 tons.

Fifteen tank-engines, six-coupled, with a radial leading axle, for passenger trains on the mountain section: diameter of cylinders, 18.8 inches; stroke, 24 inches; total heating surface, 1,302 square feet; weight loaded, 51.5 tons; smallest adhesion weight, 33 tons.

Sixteen tender-engines, with six wheels all coupled, for goods trains: diameter of cylinders, 18.8 inches; stroke 25 inches; total heating surface, 1,378 square feet; weight loaded, 61 tons; smallest adhesion weight, 38 tons. These

engines have tanks for carrying 4 tons of ballast water, to bring up the adhesion weight, if required, to 42 tons.

The building of the heavy tank-engines was subsequently suspended.

The Council of Management of the railway have pronounced the above type of tender-locomotive to be ill adapted to the railway, and the number insufficient.

In comparing the two classes—tender- and tank-engines—it will be assumed that the tank-engines have 42 tons as adhesion weight at starting, with 10 tons on the leading axle, and the tender-engines that have the same adhesion weight, with a tender weighing 11 tons empty, and 23 tons full.

The following are the advantages of the tender-engine:—(1) Simplicity, (2) accessibility of parts, (3) lower level of center of gravity, (4) greater range in choice of construction and dimensions, (5) constant load on the axle, (6) constant tractive force, (7) greater tendency to preserve the direction in case of derailment, (8) more room for water and coal, (9) consequent capability of taking a worse quality of coal, (10) less risk for men and passengers in accidents, from the presence of the tender, (11) use of strong tender-brakes.

The disadvantages are as follows:—(1) Overhang of the fire-box, causing objectionable and dangerous oscillations, (2) stiffness of the coupling between engine and tender. (3) great wear of the leading wheel flanges, (4) consequent wear of permanent way, (5) greater probability of derailment from this cause and increased cost of maintenance, (6) large difference between the total weight and the weight utilized for adhesion, occasioning either the too heavy construction of some parts, or the carrying of ballast, (7) impossibility of completely inclosing the driver's stand.

On the other hand, the advantages of the tank-engine, with free leading axle, are as follow:—(1) Secure fixing of the boiler, (2) easy traveling, (3) safety on curves, (4) low resistance on curves, (5) uniform wear of the wheel flanges, (6) reduced wear of the permanent way, (7) possibility of inclosing the driver's stand.

The disadvantages are as follows:—(1) Variable load on axle, (2) variable tractive force, (3) confined space for driver, &c., (4) difficulty of access to some parts, (5) tendency to leave the direction in derailment, (6) loss of the tender-brakes.

As regards repair and maintenance, experience shows that a tank-engine costs more than a tender-engine; but not more than engine and tender together.

On the St. Gothard railway it would not pay to burn inferior coal, as the freight is very heavy; hence the large coal space of the tender-engine is not needed. The leading bogie is not, of course, a feature of tank-engines alone, but its use is there much more easy and valuable. The question of tender-brakes has lost much of its importance now that many goods wagons have brakes, and that automatic continuous brakes are coming so rapidly into the field.

As to the efficiency of the engines, the gradient in the Kehr tunnel on the Northern division, is 2.3 per cent. The continual wetness of the rails diminishes the resistance on curves, but also diminishes the adhesion, which must not be calculated at more than one-eighth. On the south side there are gradients in the open up to 2.7 per cent., so that the adhesion is the same on both sides. The resistance may be taken as 0.005 ton per ton of engine and train. Then the greatest weight hauled will be 187 tons, giving 122 tons of train-load for the tender-engine, and 135 tons for the tank-engine. The latter will, of course, lose tractive force, as its water and coal diminishes; but it appears that when it has lost $5\frac{1}{2}$ tons its train load will still be equal to that of the tender-engine.

The consumption of fuel may be taken as for the Brenner, viz, 94 kilograms (207 lbs.) per 1,000 ton-kilometers. The weight of the trains may also be assumed as the same, say 65 tons. This, with the tank-engine, gives a total weight of 110 tons. It follows that the whole length of 90 kilometers, from Erstfeld to Biasca, might be run with 900 kilograms (1,980 lbs.) of coal, and 7 cubic meters of water, and therefore without replenishing. There must always, however, be a stoppage before entering the tunnel, and water and coal can be easily taken in at that time. The weight of the goods trains may be taken at 120 tons, or 169 tons with the engine. This would require 10 cubic meters of water and 1,400 kilograms of coal. Coal and water must therefore be taken in once

during the journey, whether tank-engines or tender-engines be used.

The lower dead weight of the tank-engine is, of course, a saving in point of fuel. It is calculated that on the mountain part of the line the saving would amount to 4,200 francs per annum. It appears, then, that tank-engines are equally efficient, safer, easier in running, and more economical, in wear and tear and in fuel, than the corresponding tender-engines. Since some tender-engines have already been ordered, it can only be suggested that half the stock should be in one form and half in the other.

As to the performance of the engines, the number of engines employed on the five main Swiss lines, in the summer of 1880, was as follows:

In service 276, or 60.8 per cent.

In reserve 71, or 16.6 per cent.

Under repair 107, or 23.6 per cent.

Taking these in round numbers as 60, 20, and 20 per cent.. it is found that the St. Gothard line will require fifty-one engines in all.

The annual mileage of the engines on the Swiss normal lines has steadily declined from 30,393 kilometers in 1874 to 24,839 in 1879. Herr Hellwag assumes that, on the St. Gothard line, the passenger engines will run 30,000 kilometers, and the goods engines 34,000 kilometers per annum, on the mountain section. The question here is the time that the driver's firemen will practically work in each twenty-four hours.

On the Swiss railways the average time is 15½ hours per day in service, of which 7½ are actual running. In Germany the figures are 17.4 and 9.6 respectively. On the French East Railway they are 10 and 5 for express trains, 10 and 6 for passenger trains, and 12 and 7½ for goods trains. On the Belgian railways the average is 10½ hours in service. For the St. Gothard railway the hours of service may be assumed to be 14, of which 6 will be actual traveling, for quick trains, and 9 for slow trains; and this for two hundred and twenty days per annum. Assuming the speed to be 22 kilometers per hour for quick trains, and 12 for slow trains, it is found that the passenger engines will run 43,000 kilometers, and the goods engines 24,000 kilometers per annum, on the mountain section of the line. On the valley sections, where the speeds are 45 and 17 kilometers, the corresponding numbers will be 48,000 and 30,000 kilometers per annum. These figures are confirmed by the mileage of certain engines on the existing Swiss railways.

To obtain a high mileage for locomotives the following are the chief points to be attended to:

(1) The engines must be properly constructed, and of good material.

(2) There must be a good distribution of the work, both for the drivers and the engines.

(3) There must be a well-equipped work-shop, to make sound and rapid repairs.

As an illustration of No. 2, the total weight taken over the Mont Cenis line in 1878, exclusive of engines, was 1,024,500 tons, or 2,807 tons per day. This was hauled by thirty-seven engines, having a total adhesion weight of 1,798 tons. Adding the tenders at 20 tons each, the total engine weight working per day was 2,538 tons, to haul only 2,807 tons of train load; in other words, the engine weight 90 per cent. of the train weight, and three times as great as the paying weight.

A table is given, which shows the amount of traffic which could be worked over the St. Gothard line by the thirty-one engines ordered, assuming their performances to be as above described. It appears that the performance of the passenger engines would be greater than that estimated as necessary on the line, but that of the goods engines considerably less. This difficulty might be overcome for a time, if the directors resist the temptation of opening the line with a large service of trains, which, in the case of a trunk line across a mountain chain, is quite unnecessary. It remains, however, that they should at once proceed with the design and construction of goods engines of a more powerful character. The type of these engines will be mainly determined by the three following conditions: (1) Utilization of the whole weight for adhesion; (2) fixed wheel-base of less than 3 meters; (3) load per axle of not more than 12 tons.

On the Austrian Southern Railway an eight-coupled engine, of 52 tons adhe-

sion-weight, hauls a train of 200 tons total weight (the maximum which has been suggested for the St. Gothard line), including 25 tons for the tender. A 70-ton tank engine, with twelve driving wheels, would haul, with a smaller consumption of fuel, about 30 per cent. more of train weight than the tender engine; in other words, would take up in three trips a weight for which the other would require four. Such a double six-coupled engine would practically haul 300 tons across the mountain, and could thus convey the maximum daily train weight of 3,250 tons in eleven trains; adding four mixed and ten passenger trains, the total number per day would be twenty-five: which could be worked without a crossing place in the great tunnel. The conclusion is that twelve-wheeled engines of this kind, more or less resembling the Fairlie type (of which three hundred have now been built) should be used for the St. Gothard line. [It is not stated how the difficulty of excessive strain on the couplings is to be got over.]

REPORTS OF ENGINEERING SOCIETIES.

THE AMERICAN SOCIETY OF CIVIL ENGINEERS.—The last number of the Transactions contains:

Paper No. 238.—Subaqueous Underpinning. By A. G. Menocal.

Paper No. 239.—The Mean Velocity of Streams Flowing in Natural Channels. By Robert E. McMath.

ENGINEERS' CLUB OF PHILADELPHIA.—The last issue of the Proceedings contains:

Paper No. 3.—Applications of Logarithms to Problems in Gearing. By Milford Lewis.

Paper No. 4.—Working Strength of Bridge Posts. By G. P. Bland.

Paper No. 5.—Thickness of Cast Iron Pipes. By P. H. Baerman.

Paper No. 6.—Resistance to Traction on Roads. Rudolph Herring.

Paper No. 7.—Philadelphia and Long Branch Railway. By C. S. D'Invilliers.

Paper No. 8.—Brick-work under Water Pressure.—By D. McN. Strauffer.

The Strength of Wrought Iron Columns. By Thos. M. Cleeman.

ENGINEERING NOTES.

THE WATER SUPPLY OF ALEXANDRIA.—Alexandria has been threatened with a water famine. Its supply is drawn from the Mahmoudie Canal, which communicates with the Nile at Atfeh. Into this canal runs also the Khatatbeh Canal, which at one time drew its supply from the Raid Canal, but now gets its water from large pumps erected last year by Messrs. Easton and Anderson, Erith Ironworks, Kent. These pumps are fixed at Khatatbeh. There are ten of Airy and Anderson's patent screw pumps, each 12ft. diameter, and capable of deilvering 144 tons of water per minute to a height of 10ft. 6in. Eight pumps are worked together, delivering 1152 tons per minute. They are driven by two pairs of compound inverted direct-acting engines of the marine type, running at 75 revolutions per minute under 65 lb. steam. There is also one reserve engine. The pumps have been working regularly since the middle of April, and were stopped about the 18th inst. in consequence of the danger to the staff employed about them. The pumps were made for the Behera Irrigation Company, for which Messrs. Easton and Co., of London and Cairo, were consulting engineers. The works were under the immediate charge of Mr. H. C. Anderson, at Cairo. On the 12th of June, a 24 hours' run gave the extraordinary high duty of 1-horse power of water lifted 3.25 meters per hour for 3.05 lb. of Welsh coal which had deteriorated considerably from long exposure to a tropical sun. The duty has ranged between 78 and 85 per cent., that is, the ratio between the work done in lifting water and the indicated horse-power. We understand that a guard has been sent out to protect Atfeh. If the works there are stopped, Alexandria will be without water, but this is not now feared.

TUNNEL UNDER THE BOSTON MOUNTAIN.—At 5 o'clock this morning the workmen of the two ends of the tunnel under the Boston Mountain, 23 miles south of this city, on the line of the St. Louis & San Francisco Railway, shook hands through the division wall. A few minutes later Mr. McDonald, the superintendent of the tunnel works, under the charge of Cameron & Holly, Col. Cameron, and Capt. Hinckley, division superintendent, passed through the aperture made by the completing blows of the workmen. Track will be completed through the tunnel in two weeks. This is the finishing stroke on the St. Louis and great Southwestern thoroughfare. The hole is 1,730 ft. in length, and is the most important work of the kind in the State. The big bridge, 800 ft. long and 123 ft. high, just south of the Boston Mountain, is also about completed. Trains from St. Louis to Fort Smith, by way of the 'Frisco, will run on the 15th of August next.

THE FORTH BRIDGE.—There has just been completed on the island of Inchgarvie, the spot where the central piers of the Forth Bridge structure are to rest, a wind gauge for the purpose of indicating the lateral pressure of the force of the wind from east to west. The erection is composed of an enormous mass of heavy timber—about fifty tons in all—which is placed upon the square tower and upwards of the old castle on the island. The top of the erection is about 100 feet above high-water level, and the apparatus upon which the wind exerts its force is a large flat screen of thick planks. This screen exposes to the wind about

200 square feet of a surface, and is mounted on small roller-wheels moving on iron rails parallel to each other. At each corner, and on both sides, are placed strong spiral springs resembling in some degree the buffer-springs of locomotives. On the east side of the screen are fixed steel wire conductors, by which the wind pressure is led to the indicator below. The apparatus is now in good working order, and the highest pressure registered since the erection is only one-fourth of the strain which the bridge is calculated to stand.

THE PANAMA CANAL.—The latest reports from the Isthmus are again rose-colored, or intended to be so. The line of the canal through the virgin forest has been almost entirely cleared. The great cutting of the Cordilleras, at the highest point of its course, has been begun. The Couvreux excavators are in operation—it is said, much to the suprise of the Americans, who had predicted that they would not work. It seems to be considered a matter for great congratulation that the death-rate has fallen below 70 per 1000, at which figure it had long stood; and the sanitary condition of the employes is held to be much improved.

IRON AND STEEL NOTES.

IRON AND STEEL PRODUCTION IN RUSSIA.—The production of pig-iron was, in—

1874	23,212,772	puds.
1875	26,061,323	"
1876	26,956,350	"
1877	24,403,319	"
1878	25,472,540	"
Average	25,221,360	

and in 1879, 26,412,806.

The quantity of steel turned out rose in—

1874	to 526,778	puds.
1875	" 789,253	"
1876	" 1,093,719	"
1877	" 2,702,863	"
1878	" 5,801,754	"
Average	2,182,873	

and in 1879, to 12,929,170

The production of pig-iron was, therefore, increased by 940,266 puds, with respect to the previous year's returns, and by 1,191,446 puds, as compared with the average of the five-yearly period.

In wrought iron there was an increase of 432,125 puds over the figure of the previous year, and a diminution of 361,423 puds from the average of the years from 1874 to 1878. This diminution is the natural consequence of the rapidly increasing production of steel, which has made extraordinary progress, especially at St. Petersburg, in Poland, in the Oural, and in the Brjansk establishments.

The year 1879 shows, for steel, an increase of 7,127,416 puds over the figures of the preceding year, and of 10,747,297 puds over the five-yearly average. This increase in the steel manufacture is almost entirely due to the numerous orders for the State railways, and to the premiums granted by the Government.

The manufacture of wrought iron and steel barely amounts to half the demand. To form a just idea of the measure in which the production is inferior to the consumption, it is sufficient to call to mind the quantity of rails necessary for the construction and repairs of the iron ways of the Empire. In 1879, there were 21,841 versts of railway opened, without counting the sidings. Besides, the Russian railway system receives marked additions every year; and the double line of way is coming generally into use. Besides rails, large quantities of iron and steel are absorbed in the construction of bridges, the fixing of the rails, the rolling stock, and in buildings.—*Journal of Society of Arts.*

YIELD OF STEEL PLATES.—The steel department of the Dalzell Iron and Steel Works, at Motherwell—Mr. David Colvill's—continues taxed to its utmost capacity in the manufacture of ship and boiler-plates, beams and bars. The yield on occasional shifts reaches astonishing figures. The slabbing hammer is a fine powerful tool capable of giving a blow exceeding 400 foot-tons, and is worked in connection with three gas heating furnaces. The plate rolling mill has two pairs of 28in. rolls by 8ft. long, and is driven by a magnificent pair of Ramsbottom reversing engines. Two large gas furnaces heat the slabs for this mill. The following figures from Mr. Colville's books give the material charged and the finished ship and boiler-plates yielded during two succeeding shifts of twelve hours each on the 9th inst.:—Hammer: Day shift, ingots charged, 73 tons 7 cwt, 3 qr.; slabs and billets produced, 67 tons 0 cwt. 3 qr. Hammer: night shift, ingots charged, 79 tons 0 cwt. 2 qr. 21 lbs.; slabs and billets produced, 73 tons 14 cwt. 2 qr 21 lb. Plate mill: day shift, slabs charged, 66 tons 0 cwt. 3 qr. 69 lb., finished plates yielded, 52 tons. 5 cwt. 0 qr. 3 lb. Plate mill: night shift, slabs charged, 67 tons 13 cwt. 1 qr. 23 lb.; finished plates yielded, 52 tons 3 cwt. 1 qr. 3 lb. With a single hammer and plate mill worked with a similar furnace power this production has never, we believe, been surpassed.

ORDNANCE AND NAVAL.

THE BRITISH NAVY.—A parliamentary return just issued shows the amount of shipping—tons weight of hull—estimated and built from the year 1865-6 to the year 1881-2 for the British navy. The total number of ironclads, and wooden, iron, and composite vessels actually built during that period in her Majesty's dockyards and by contract amounted to 322,952 tons, to the value of £15,174,690. The smallest quantity of shipping built in any one year during that period was 13,566 tons in 1866-7, and the largest quantity in the year following, when 27,422 tons were built. The greatest value represented by the shipping constructed in one year was in 1876-7, when £1,423,418 were expended in the construction

of 24,230 tons of shipping, principally composite vessels. The return also includes a statement of the amount of money proposed to be spent on labor, and that actually spent on the several ships building in her Majesty's dockyards during the year 1881–82, showing the corresponding tonnage. For armored ships the amount proposed to be spent was £339,357, and that actually spent £350,535, upon a tonnage actually built of 10,748. For unarmored vessels, the amount proposed to be spent was £137,956, and that actually spent £169,939, upon 4690 tons actually built. The amount of unarmored ships proposed to be built by contract during 1881–2 was 4050 tons, at an expenditure of 220,645; the amount actually built was 3172 tons, for which £194,119 has been paid. There were no armored ships built by contract during that period.

STEEL FACED ARMOR PLATES.—Some recent trials have been made of steel-faced armor plates for the protection of the *Collingwood*, now under construction at Pembroke. In our issue of January 20, we recorded the results of the testing of a plate measuring 8 feet in height by 6 feet in breadth, with a thickness of 11 inches, of which the steel face was 3¾ inches. It was constructed according to Wilson's process, and was fired at three times by the 9-inch 12-ton gun on board the *Nettle* at Portsmouth. Of the few cracks which were produced by the impacts, only two extended to the edge of the plate, and none went beyond the depth of the steel face, so that the 7¼ inches of iron backing remained whole and unbroken at the end of the ordeal. The maximum penetration worked by the 250-pound projectile was 4.7 inches, while the bulges at the back never exceeded five-eighths of an inch. The hardness, toughness, and resistance of the plate were such that it was felt that the 9-inch gun had ceased to be an adequate test, and it was accordingly resolved not only to make use of the 10-inch 18-ton gun in all subsequent armor testing, but to subject the already injured Cammell plate to an attack from the larger caliber. This wholly exceptional trial took place recently at Portsmouth, but the results of the firing were only ascertained on Tuesday of last week, when the plate had been removed from the bulkhead. In order that the tremendous character of the second ordeal may be fully understood, it may be mentioned that the initial velocity of the 9 inch projectile propelled with a battering charge of pebble powder is 1420 feet per second, and that its energy at the muzzle is 125 foot tons per inch of circumference. The projectile of the 10-inch gun, on the other hand, while it has a slightly less initial velocity, or 1364 feet per second, has an energy of 166-foot tons. It was thought that one shot from the large gun at 30 feet would be sufficient to complete the disintegration of the plate, and, as a matter of fact, so confident were the gunnery officers that a second round would not be required that only one shot and charge were brought from below. The projectile hit the target about a foot below the indent inflicted by the second shot, at the previous trial, and at equal distances from the right and lower edges. To the surprise of everybody the impact had apparently no effect whatever upon the plate, no new cracks being produced, while the old ones remained precisely as they were. The head of the shot remained imbedded in the plate. Three more shots were discharged at the plate near the margin. Nos. 2 and 3 developed former cracks only, while the last round caused a new crack to appear, extending from the indent to the edge. In no instance, however, was the plate cracked through, the injury stopping short at the point where the steel face is welded to the iron backing. To all appearances the plate has suffered little injury from the second bombardment, and it is a remarkable circumstance, and at present wholly inexplicable, that, while the 9-inch gun made an indent on the surface of the plate 4.7 inches deep, the heavier gun with its increased striking energy only penetrated 4.4 inches. On the plate being taken from the bulkhead it was found that the bulges resulting from the first trial in January were five-eighths of an inch, while the bulges produced by the 10-inch shot were one inch and one sixteenth in extent. In both instances the curvature of the surface was free from cracks. This plate is the most successful which has yet been tested at Portsmouth, and the result of the severe ordeal through which it has passed will probably reopen the question as to the expediency of superseding the old protection of our men-of-war by the new compound armor.—*Engineer.*

TWIN-SCREW STEAMERS FOR THE GOVERNMENT OF THE ARGENTINE REPUBLIC.—In November of last year the Consul General of France for the above republic entered in a contract with Messrs. Edwards and Symes, shipbuilders and engineers, Cubitt Town, London, E., for the construction of four iron, light draught twin-screw steamers On the 20th of May the first of these steamers—which is named *La Capital*, 85 ft. long, 15 ft. beam, and 7½ ft. deep, with raised quarter-deck and forecastle—being nearly completed, proceeded down the river to the measured mile at Long Reach for her first official trial trip, and although the weather was very unfavorable for the trial of such a light draught vessel, yet she came up to every expectation of her builders, who deserve hearty congratulations on the results of her trial trips The mean draught of water was under 3½ ft., and mean speed obtained on six consecutive runs being as near as possible 11¾ knots. On the 8th inst., she again proceeded down the river for her second official trial trip, having been loaded with twenty two tons of cargo, making her mean draught of water 4 ft. Under these conditions the mean speed attained on six consecutive runs was 11 knots, thus more than fulfilling the expectations of her builders, and the contract speed. The propelling machinery is composed of two ordinary independent compound surface condensing engines with high-pressure cylinder, 11 in. in diameter, and low-pressure 20 in. in diameter, each set driving a screw 4 ft. in diameter. The engines are supplied with steam from an ordinary marine return tube boiler, which maintained a pressure throughout the trials of 90 lb.,

driving the engines 195 revolutions per minute, the vacuum in both condensers being 26 in., the whole of the machinery working well during the whole time the vessel was under steam. The second vessel of the four ordered, which is the first of a smaller class of the above type, will proceed down the river next week for her first official trial, the results of which we shall give at a future date. The builders have lately constructed two beautifully fitted yachts, and besides the above four have now in hand building a fire engine tug-boat, three cargo steamers, a paddle steamer, besides several smaller craft and steam launches.—*Engineer.*

A Novel Atlantic Steamer.—We learn that a Swedish engineer, Captain Lundborg, has just concluded an agreement with Messrs. Charles L. Wright & Co., of New York, for the construction of a fleet of steamers, built on Captain Lundborg's patent, to run between New York and Liverpool. The inventor alleges to have founded a new basis for the construction of fast-going vessels; in fact, he asserts that a vessel of his type will run close upon 21 knots per hour, and thus accomplish the passage across the Atlantic in 5½ days. The dimensions of the vessel are:—Length, 450 feet; greatest width, 66 feet; draught, when loaded, 23 feet. Her weight is 10,881 tons, and she will be driven by four engines of 4500 horse-power each, working two propellers, as, according to the inventor, the high rate of speed which he aims at cannot be obtained by only one. The vessel will be built entirely of steel, with a false bottom, and watertight compartments of a novel cellular form. The proportion between the length and breadth of the ship is 7 to 1, instead, as is the case with steamers now in use, of 10–11 to 1, and which the inventor states will increase her strength. Above the water line she will not exhibit any remarkable appearance, but the submerged part of the hull is entirely different in construction to anything before tried in shipbuilding, the widest part, 15 feet to 16 feet, being far under the surface and ending aft horizontally. The propellers run in the vessel's hull, and not, as usual, on shafts outside it. Another feature distinguishing Captain Lundborg's construction is the bow of the vessel, which is sharpest at the water line—quite the reverse of what is the case with vessels at present in use—and broadens downwards to the keel, a circumstance which, it is stated, will add to the stability of the vessel and prevent lurching. There will be two rudders steered simultaneously, and the propellers are fixed behind them. The construction of the first steamer is to be commenced at once at Washington. She is to accommodate 600 first and 1000 second and third class passengers, whilst carrying 2700 tons of coals and 550 tons of goods. It is expected that about a year and a half will be required for building the vessel.—*Iron.*

Trials of Machine Guns.—Captain Codrington and the gunnery staff of Her Majesty's ship Excellent have recently been occupied with final experiments in connection with machine guns, and more especially with a view of testing the efficacy of several naval carriages and mountings proposed for machine guns. The trials were held on board the Excellent and also upon Whale Island, in Portsmouth Harbor. A new mounting was tried for the Nordenfelt 2-pounder gun of 1½ inch caliber, as the mounting previously adopted was found too light to secure the desired accuracy. The new mounting was ascertained to be eminently satisfactory, as will be seen from the results of the firing. Ten shells fired for accuracy with deliberate aim between each shot gave, at 300 yards range, a mean deviation from the point of impact of only 5¾ inches. Seven out of the 10 shots hit the bull's-eye, while the least favorable of the other three hits was only three inches below the bull's-eye. The gun was then fired for a minute for accuracy, combined with rapidity. With a comparatively slow aim, 12 shots only were discharged during the time, but of these four were bull's-eyes and eight inners, the mean deviation being six inches. The next trial was to fire at 300 range for a couple of minutes, against two targets, 120 feet apart, and at different levels, changing the aim from one target to the other between each shot. Twenty-four rounds were fired in the two minutes. One missed the target in consequence of its being fired before the gun was laid. Of the 23 hits, three bull's-eyes, six inners, and three magpies were scored on the right target, and four bull's-eyes, five inners, and two magpies were scored on the left target. There were no outers. The new mounting was thus proved to do perfect justice to the gun, which at previous official trials, as from time to time reported in these columns, has given great satisfaction. With its high initial velocity of 1740 feet per second it has penetrated a 1½ inch steel plate, or 2¼ inches of iron, at 300 yards; and it has fired as many as 29 shots in one minute without deliberate aiming. The weight of the gun is 3 cwt., and it has been tried with solid steel projectiles, as well as chilled and common shells. A new system of bulwark mounting was afterwards tested at the request of Mr. Nordenfelt, who had sent down three separate naval bulwark carriages suitable for rifle caliber machine guns. These consisted of a carriage for the heavier guns, such as the Gardner 5-barreled and the Nordenfelt 10-barreled guns, weighing respectively 2½ cwt. and 2 cwt.; a carriage for medium weight machine guns, such as the Gardner two barreled and the Nordenfelt 5-barreled guns, each weighing about one cwt.; and a bulwark carriage for light machine guns, such as the Gardner one-barreled and the Nordenfelt 3-barreled, each of which weighs half a hundred weight. These bulwark mountings were made on the same lines as the carriage used by the Navy for the Nordenfelt 1-inch gun with screw motion, by means of band wheels for elevating as well as traversing. The 10-barrel Nordenfelt gun on the heavier mounting, when firing at 300 yards 10 rounds from one barrel without adjusting the aim between the shots, gave a mean deviation of 6½ inches. Of 100

rounds fired rapidly 83 hit within a quadrangle of 7 feet by 5 feet. The five-barreled Nordenfelt gun fixed on the medium weight mounting, gave, at 300 yards, 5½ inches mean deviation for 10 shots fired without adjustment of aim; and of 50 fired rapidly 34 shots fell within a quadrangle of 8½ feet by 6 feet. Tested in the same manner on the light mounting, the 3-barrel Nordenfelt gave a mean deviation of 9 inches out of 10 shots; while 28 projectiles out of 39 fired hit within a quadrangle of 7 feet by 6 feet, eight of the hits being bull's-eyes. The three representative mountings were next tested for strength and stability. The 10 barrel Nordenfelt gun fired 3000 rounds in 3 min. 3 sec.; the 5-barreled fired 1000 rounds in 1 min. 41 sec.; and the 3-barrel gun fired 390 rounds in 1 1-3 min. After this very severe test the carriages were found to have lost none of their steadiness and rigidity, while the guns, as well as their carriages, worked at the end without more exertion than at the beginning. The guns had neither been cleaned after the accuracy trials, nor cleaned or oiled during the rapid firing. The 10-barreled gun had one misfire out of 3000, and the other guns had five misfires out of 1390 rounds. The feeding and extraction of all the guns worked without a hitch or jamb of any kind, and the same man fired the whole of the 4390 rounds without difficulty. The whole of the guns used the same old service Gatling cartridges as were used at Shoeburyness in 1881, before the cartridge rims were thickened to suit the Gardner guns. In order to test the convenience of the new carriages for following moving objects, the guns were fired at alternate targets 120 feet apart, changing target between each discharge, the gun being in each instance laid 45 deg. off the targets and 10 deg. below the level of the targets. The time of laying the guns on the first target was counted within the half-minute allowed for each gun. The 10-barrel gun on the heavier mounting gave an average of eight volleys (80 shots), the 5-barrel, 11 volleys, and the 3-barrel, 12 volleys in the half-minute. The 5-barrel gun was fired from a special masthead mounting provided, in addition to the three mountings previously used. One hundred rounds were fired in 10 seconds, without deliberate aiming, at 300 yards, 59 shots hitting a target 12 feet by 6 feet. One hundred rounds were afterwards fired in 27 seconds, with deliberate aiming between each volley, when 64 shots hit the target. The 1-barrel gun, weighing 16 lbs., was fired from a light portable deck carriage, with the gun only 2 feet above the deck. The first 30 rounds were fired in 11½ seconds, and the second 30 rounds in 10 seconds—equal to a rapidity of fire of 180 rounds per minute. Five thousand five hundred rounds of Gatling cartridges in all were fired without any hitch, thus showing that Mr. Nordenfelt has entirely overcome the disadvantages in feeding and extracting rifle cartridges which were remarked upon by the Committee of Machine Guns in 1880 and 1881 at Shoeburyness.—*Iron*.

RAILWAY NOTES.

THE total number of deaths and injuries reported by the railway companies to the Board of Trade during the year 1881 is given in the following table:

	Killed.		Injured.	
	1881.	1880.	1881.	1880.
Passengers—				
Accidents to trains, &c.	23	28	993	905
Accidents from other causes	85	114	867	709
Servants—				
Accidents to trains, &c.	19	23	168	118
Accidents from other causes	502	523	2278	1962
Level crossings	83	74	32	30
Trespassers, including suicides	328	330	131	156
Other persons	56	43	102	79
Total	1096	1135	4571	3959

In addition to the above—One passenger was killed and 112 injured whilst ascending or descending steps at stations; forty-four injured by being struck with barrows, falling over packages, &c., on station platforms; thirty-six injured by falling off platforms; and two killed and sixty injured from other causes. Of servants of companies or contractors, six were killed and 963 injured whilst loading, unloading, or sheeting wagons; one was killed and 303 were injured whilst moving or carrying goods in warehouses, &c.; five were killed and 172 injured whilst working at cranes or capstans; fourteen were killed and 239 injured by falling off platforms, ladders, scaffolds, &c.; eight were killed and 576 injured whilst working on the line of its sidings; and one was killed and 231 were injured from various other causes. Nine persons who were transacting business on the companies' premises were also killed, and 119 were injured—making a total in this class of accidents of fifty-three persons killed and 4015 injured. The total number of personal accidents reported to the Board of Trade by the several railway companies during the year amount to 1149 killed and 8676 injured. For 1880 the total was 1180 killed and 6692 injured.

THE Northern Railway Company of France is making a series of experiments with a view to demonstrate that automatic action of continuous brakes is not indispensable to stoppage of the tail of a train in case of rupture of the couplings in course of the ascent of a hill. On rising and falling gradients the stoppage of the tail of a train has been effected with the vacuum brake by means of the communication cord connecting the engine with the rear wagon, where there must apparently be another or second brake. At the moment of rupture of this cord intentionally caused the brake is set free by the descent of a counterbalance weight, and the tail of the train stopped. The experiments yet made have

been between Paris and Lille, in presence of engineers from the Northern and the Belgian State Railways, and are to be continued. The *Moniteur Industriel* says the Belgian engineers have asked for a fresh trial with the train running down a gradient on the line between Paris and Montsoult.

THE Swiss *Railway Gazette*—the *Eisenbahn* of Zurich—reports that the Heberlein automatic friction brakes, which were introduced on trial on the Berne-Chaux-de-fonds line about five months since, "have given such thoroughly satisfactory results that the direction of the Jura Berne Lucerne Railway has decided on the gradual adoption of these brakes; and as a commencement, the express and passenger trains on the Berne Lucerne line are being fitted up in readiness for this season's traffic. By the adoption of these powerful brakes, which admit of stopping trains more quickly at the stations and of descending steep inclines at greater speed, a considerable acceleration of the train service can be secured, which, in the case of the Berne Lucerne line —which is 95 kilos. long and has seventeen intermediate stations and inclines of 1 in 50— will amount to a reduction of half an hour in a journey of three hours and a-half. It results from the above that continuous brakes are not only valuable in the case of express trains, but also more especially in that of such passenger trains as have to stop frequently at stations only short distances apart, and which consequently run very often between the stations with even a greater speed than the actual express trains." The Heberlein brake has undergone important modifications since we illustrated it in our columns, and is daily making important progress on numerous railways, chiefly on the Continent. On the Royal Prussian railways a large quantity of new stock is being fitted with the Heberlein automatic brake, and the Imperial German Board of Control for Railways seems to be wholly in favor of this mechanical brake, instead of brakes using vacuum or air pressure.

IN a paper recently read before the Institution of Civil Engineers in Ireland, entitled "Engineering Notes in Ceylon," by H. F. A. Robinson, the author says:—"The center of Ceylon is mountainous, and it is only of late years that a trace was discovered by which a railway could be brought up to Kandy from the low country. As it is, the line runs for about fifty miles nearly level, and then ascends for twelve miles at a uniform gradient of one in forty, with curves as sharp as five and a-half chains. Two engines are necessary to take the train up this pass, and the time for the distance is over an hour. Coming down, brakes are applied to every car separately, which, as may be imagined, has the effect of greatly shortening the life of the rolling stock. The gauge of this line is 50 ft. 6 in., or the ordinary Indian gauge. The sleepers, which are all imported, are creoseted, which, besides improving the sleeper, renders it impervious to the ravages of white ants. The carriages are very similar to those in ordinary use at home, although they are better ventilated; but they are very stuffy and uncomfortable, and, in fact, not fit for the climate. American cars would be much more suitable for the European passenger traffic, as they have thorough ventilation, which is so necessary in the East."

BOOK NOTICES.

PUBLICATIONS RECEIVED.

AN EPHEMERIS OF MATERIA MEDICA, PHARMACY, THERAPEUTICS AND COLLATERAL INFORMATION. By E. R. Squibb, M. D.; E. H. Squibb, S. B., M. D.; C. F. Squibb, A. B., Brooklyn.

PROFESSIONAL PAPERS OF THE SIGNAL SERVICE.

No. 2. Isothermal Lines of the United States; 1871-80. By Lieut. A. W. Greely.

No. 3. Chronological List of Auroras; 1870-79. By Lieut. A. W. Greely.

No. 5. Construction and Maintenance of Time-Balls. Prepared under direction of Brvt. Maj. Gen. W. B. Hazen.

No. 6. Reduction of the Pressure Sea Level. By Henry A. Hazen, A.M.

MONTHLY WEATHER REPORT FOR MAY.

TRANSACTIONS OF THE AMERICAN SOCIETY OF MECHANICAL ENGINEERS.

PROCEEDINGS OF THE ENGINEERS' CLUB OF PHILADELPHIA.

AMERICAN JOURNAL OF MATHEMATICS, Vol. 4, No. 4.

EFFICIENCY OF STEAM ENGINES AND CONDITIONS OF ECONOMY. By Robert H. Thurston, A.M., C.E.

THROUGH the kindness of Mr James Forrest, Secretary of the Institution of Civil Engineers, we are in receipt of the following valuable papers of the Institution:

Lancaster Waterworks Extension. By James Mansergh, M.I.C.E.

Bridges in New Zealand. By Robert Hay, A.M., I.C.E., and Harry P. Higginson, M.I.C.E.

The burning of Town Refuse at Leeds. By Charles Slagg, A.M., I.C.E.

Canal Navigation in Belgium. By A. Gobert.

The Rokuzo River Bridge. By Richard Vicars Boyle, M.I.C.E.

New York Elevated Railroads. By Robert Edward Johnston, M.I.C.E.

Light Scaffolding. By John Cundy, A.M., I.C.E.

The Design of Structures to Resist Wind Pressure. By Charles B. Bender.

The resistance of Viaducts to Sudden Gusts of Wind. By Jules Gaudard (Republished in this Magazine).

Steel for Structures. By Ewing Matheson, M.I.C.E.

The Theory of the Gas Engine. By Dugald Clerk (will be republished in the present volume of this Magazine).

HOUSE DRAINAGE AND SANITARY PLUMBING. By Wm. Paul Gerhard. Providence: E. L. Freeman.

This is the best contribution to practical sanitary science that we have yet seen. The author clearly specifies the objects to be accomplished, and then in the most elaborate manner describes the best approved mechanical appliances devised for such accomplishment.

The illustrations are very numerous and very good.

We shall shortly republish a large portion of this essay in this Magazine.

ELEMENTARY DECORATION. By James William Facey, Jun. London: Crosby Lockwood & Co.

But few subjects attract more general attention at present than decoration. Only the rudimentary principles of house decoration are here aimed at, but the book is well filled with useful information. The illustrations are numerous and varied, and relate not only to decorative forms but the place and method of application.

This book is No. 229 of the well-known Weale's Series.

A SCHOOL COURSE ON HEAT. By W. Larden, M.A. London: Sampson Low, Marston & Searle.

The author informs us in a brief preface, that the book is intended to supply a want felt by many who are teaching the subject of heat to such classes as those in the English public schools. And furthermore that the chief characteristics of the book are:

1st. That the reasonings and explanations are at first very elementary; brevity being only gradually attained.

2d. The writer has introduced collateral subjects for the purposes of elucidation.

3d. The mathematical parts are carefully treated, and typical examples are worked out.

4th. Questions on the subject matter of each chapter are given at the end of it.

5th. A shorter course than that presented by the whole book is found quite completely given by the omission of certain marked sections.

The typography is very good, and the illustrations, about 120 in number, are of excellent character and well adapted to the text. This book will do its best service with students who are working without the aid of a teacher.

THE MILITARY TELEGRAPH DURING THE CIVIL WAR IN THE UNITED STATES. By William R. Plum, LL.B. New York: D. Van Nostrand.

The object of this work is to show the valuable services rendered by the Military Telegraph Corps in the late Civil War. In order to illustrate the importance of the Telegraph, and give it its due setting, it was considered necessary to give a running account of the struggle itself. In this the author has been greatly aided by important telegrams, and other papers, official and otherwise, which have never been published, and by many Southern operators who have furnished interesting and important facts from their point of view. The author has striven to be accurate and just; avoiding debatable questions, and seeking concisely to state material facts.

The ancient and modern methods of communication are explained; also the Federal and Confederate cipher system.

The work consists of 2 octavo volumes with a total of 767 pages, with portraits and illustrations.

THE BOILER-MAKER'S READY RECKONER. By John Courtney; Revised by D. Kinnear Clark, C.E. London: Crosby Lockwood & Co.

This is but little more than a book of convenient tables for the boiler maker. Enough practical geometry precedes the tables to instruct the artisan in the method of laying out his work.

The tables afford the piece-work plater who is paid by the ton, how to find the weight of his iron when he has the size of it. Riveters may reckon the payment of the holder on from the rivet table. Smiths may get information in regard to circumferences of circles of angle iron and plate iron.

The work is designed to save much vexatious and intricate work to the artisan of riveted iron structures.

REPORT OF THE SOLAR ECLIPSE OF JULY, 1878. By Cleveland Abbe. Washington: Government Printing Office.

This Report forms No. 1 of the Professional Papers of the Signal Service.

Chapter I. is chiefly devoted to the instructions issued for the benefit of observers along the line of totality.

Chapter II. details the operations of the Signal Service Expedition to Pike's Peak, and is the more important part of the Report.

Chapter III. is a collection of the miscellaneous observations and reports to the number of eighty.

Chapter IV. gives a summary of results.

A large number of sketches of the corona are appended.

RAILROAD ECONOMICS. SCIENCE SERIES, No. 59. STRENGTH OF WROUGHT IRON BRIDGE MEMBERS. SCIENCE SERIES, No. 60. By S. W. Robinson, C.E. New York: D. Van Nostrand.

Our readers have already had an opportunity of judging of the merits of these two treatises, as they are both reprints from the Magazine.

The first one contains two topics quite of an original character and of undoubted value to railway engineers: The Bridge Indicator and Easement Curves.

In Part II. of the second one is found an exceedingly concise compendium of Practical Formulas for Beams, Struts, and Columns.

ELECTRIC LIGHTING. Translated from the French of Le Comte Th. Du Moncel. By Robert Routledge, F.C.S.. London: George Rutledge & Sons.

This work is well designed to meet the wants of those who profess only a general knowledge of physical science, and who desire to understand the relative merits of the many so-called systems of Electric Lighting.

Part I. After a brief historical sketch of pub-

lic electric lighting, the author defines the terms necessarily used in discussing the comparative merits of the various modern magneto-electric and dynamo machines.

Part II. Describes the generators of electric currents for the production of light, taken in the order of their invention. This leads to a full description of the various magneto machines with their theory of action.

Part III. Gives full descriptions of the Electric Lamps including their regulators.

Part IV. Deals with the economic question of cost of Electric Lighting.

Part V. Discusses the actual and probable applications of the Electric Light

The original work gives us the state of progress down to 1880. An appendix by the translator gives descriptions of the later lamps.

The illustrations are numerous.

LINEAR ASSOCIATIVE ALGEBRA. By Benjamin Peirce, LL.D. New York: D. Van Nostrand.

The number who will read this work and attain a thorough understanding of it is certainly quite limited. But of the mathematical students who in studying it will reap great benefit through the more expanded views of mathematical research they will gain, the number is, without doubt, very great.

It is the work of one of the first mathematical minds of our day, and only accomplished mathematicians can tell us how valuable it is.

Lithographed copies of the treatise were distributed by the author among his friends in 1870. It was printed first for the American Journal of Mathematics. The present edition is a new one, with addenda and notes by C. S. Peirce, the son of the author.

The book is a quarto of 133 pages and is beautifully printed.

MISCELLANEOUS.

M. BREMOND states as a general law that, by reason of rarefaction of air, "gas loses at least one liter of illuminating power per 50 meters of altitude." He give the details of an interesting experiment made on the Northern Railroad of Spain, observations being taken at various altitudes on the way from Madrid, 595 meters above sea-level, to La Canada, a station 1373 meters above sea-level. The following table, in which Paris is taken as a unit of comparison, gives some of the results of his experiments:

City.	Altitude, meters.	Barometric pressure, millimeters.	Illuminating power.
Paris.....	0	0.754	105
Vienna...	68	0.747	103
Moscow...	235	0.732	99
Madrid....	573	0.705	87
Mexico....	2212	0.572	30

FROM a recent work on "Metal Alloys," published in Germany, the author, Mr. Guetlier, gives a few suggestions on the subject of fusing the metals, with which the *Jewelers' Journal* prefaces the recipes selected. (1) The melting pot should be red-hot—a white heat is better—and those metals first placed in it which require the most heat to fuse them. (2) Put the metals in the melting pot in strict order, following exactly the different fusing points from the highest degree of temperature required down to the lowest, in regular sequence, and being especially careful to refrain from adding the next metal until those already in the pot are completely melted. (3) When the metals fused together in the crucible require very different temperatures to melt them a layer of charcoal should be placed upon them, or if there is much tin in the alloy a layer of sand should be used. (4) The molten mass should be vigorously stirred with a stick, and even while pouring it into another vessel the stirring should not be relaxed. (5) Another hint is to use a little old alloy in making new, if there is any on hand, and the concluding word of caution is to make sure that the melting pots are absolutely clean and free from any traces of former operations.

IN the opinion of Herr W. Hempel the hardening of vulcanized india-rubber, which takes place with piping and other goods after a short period of use, is caused by the gradual evaporation of the solvent liquids contained in the india-rubber, and introduced during the process of vulcanization. Herr Hempel has made experiments for a number of years in order to find a method of preserving the india-rubber. He now finds that keeping in an atmosphere saturated with the vapors of the solvents answers the purpose. India-rubber stoppers, tubing, &c., which still possess their elasticity are to be kept in vessels containing a dish filled with common petroleum. Keeping in wooden boxes is objectionable, while keeping in air-tight glass vessels alone is sufficient to preserve india-rubber for a long time. Exposure to light should be avoided as much as possible. Old hard india-rubber may be softened again by letting the vapor of carbon bisulphide act upon it. As soon as it has become soft it must be removed from the carbon bisulphide atmosphere and kept in the above way. Hard stoppers, the *Journal* of the Society of Chemical Industry says, are easily made fit for use again in this manner, but the elastic properties of tubing cannot well be restored.

W. SPRING has shown that, when a mixture of bismuth filings, cadmium, and tin, in the proportions necessary for the formation of Wood's alloy, is subjected to a pressure of 7,500 atmospheres, the mass thus obtained powdered and again subjected to the same pressure, a metallic block is formed which has all the physical properties of the alloy. Its specific gravity, color, hardness, brittleness, and fracture are the same; and when thrown into water heated to 70 degrees it melts at once. In like manner Rose's metal was made by subjecting the proper mixture of lead, bismuth, and tin to high pressure. If zinc and copper filings are repeatedly subjected to pressure, a mass resembling brass is finally obtained. If, however, on the other hand, the attempt is made to "squirt" brass, zinc and tin will be squirted, and the copper remain.